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NOTES AND COMMENTS

Sir William Flower

In consequence of failing health, Sir William Flower has found it necessary to resign his position as Director of the Natural History Branch of the British Museum. Both the cause of the resignation and the resignation itself will be deeply regretted by all naturalists in this country and abroad. In the charming volume of Essays, reviewed on another page of this number, Sir William tells us how he has been, from childhood upwards, a museum man. With the great expansion that followed on the removal of the natural history collections from Bloomsbury to South Kensington, Sir William found his opportunity, and he has availed himself of it to the full. To his staff he has been an inspiration and an example. Not one, not even the youngest, of his energetic and enthusiastic helpers can be said to have shown a more open mind, more desirous of proving all things and holding fast to that which was best for the display and arrangement of those wonderful collections. But there are other ways in which the head of an important institution of the State can make his influence felt, and Sir William's zeal for his Museum was never at rest. To urge the claims of the establishment on an unwilling Treasury, or (yet harder task) to extract sympathy from the ranks of power, wealth, and fashion, these formed the employment of what might have been his leisure hours. The high standing of our Natural History Museum, as well as the improvement in the character and position of its staff, are largely due to his personal exertions. This, we rejoice to see, has been heartily recognised by the Trustees, and the "sincere good wishes" which they have offered through Lord Dillon, as chairman of the Standing Committee, will be shared by the many who have, in one way or another, come into contact with Sir William Flower.

The Directorship of the Natural History Museum

The question of Sir William Flower's successor has for some time past exercised the minds of British naturalists, and the names of many more or less eminent men have been mentioned in that con-

nection. Sir William's retirement, we may remind our readers, creates two vacancies, namely, the Directorship of the Natural History Section of the British Museum, and the Keepership of the Zoological Department, the duties of the latter post having been taken over by Sir William on the retirement of Dr Günther. For the present Dr Henry Woodward is acting as Deputy-director, while the work of the Zoological Department is presumably in the hands of the Assistant-keepers. Yet a third question suggested itself to many, owing to the adoption by the Principal Librarian, under a recent Treasury scheme, of the title Director. This was supposed to show that the Trustees wished to bring the whole of the British Museum more immediately under one head than had been the case since the removal of the natural history collections from Bloomsbury. These fears were confirmed by a prompt official *démenti*.

Against any action that might lessen the independence of the Natural History Museum an energetic protest was soon raised, and a memorial signed by many leading men in various branches of science, and by others, was stated by the public press to have been addressed to the Trustees. The memorialists considered that the principal official in charge of the natural history collections should not be subordinate to any other officer of the Museum; he should meet the Trustees and represent them before Her Majesty's Treasury as the responsible head of a department and not as a subordinate. It is clear, the memorialists pointed out, and their contention was emphasised by a leading article in the *Times*, that the interests of the Departments at Bloomsbury are totally different from those of the Natural History Museum, and that the special knowledge and sympathies and individual museum experience that fit a man for the post of Principal Librarian militate against his caring adequately for the wants of natural history. The *Times* further observed that the proposed action would deprive naturalists of one of those very few posts "to which they might reasonably look forward as a reward for study and research; and we all know that . . . the prospect of reward may serve to keep an able man steadfast to a pursuit which he might otherwise be tempted to forsake for some other and more promising mode of activity." The inducements to enter the service of the Natural History Museum are, it has been stated, none too high from a pecuniary point of view, and the suggestion appears to be that the Trustees are far more likely to obtain a good class of assistants in future if they let it be seen that the highest posts are not excluded from those who have gained knowledge and experience in a long and devoted service.

Other opinions of interest and originality were elicited by the newspaper discussion: such as, that a botanist is not a naturalist, that Mr Du Cane Godman is merely a collector of insects, that a

voyage to Bering Straits does not necessarily fit a man for the administrative work of a large government department, that the Primate cannot tell a Painted Lady from a Camberwell Beauty, that a man who cannot teach the Trustees how to suck eggs is not fit to direct a Museum of Natural History. However these things may be, the recommendations of the Standing Committee have been laid before the three Principal Trustees, the three have adjourned the discussion, and criticism of their action must therefore be reserved for our next number.

The Museum of 'Practical' Geology

We learn from *Nature* that 500 Fellows of the Geological Society signed a memorial to their president and council protesting against the transference of the Museum of Practical Geology to South Kensington, since "removal of the collections would seriously impede the progress of science, especially on its economic side." The Council did not see its way to comply with the request of the memorialists, that it should address the Government on the subject; indeed, it expressed the opinion that the question of the removal required more consideration than it appeared to have received. Certainly this laying stress upon the 'economic side' of the question is a trifle ridiculous. As a well-informed article in the *Builder* of June 25th points outs, economic geology is the one thing that is lacking at the Museum of Practical Geology. The collection of building-stones is very incomplete, and even that is unaccompanied by the necessary particulars. "The clay-working industry," says our contemporary, "is not much advanced by the miserable show at present arranged in the Museum." Agriculture, and even mining are but indifferently attended to. If a museum "with the special object of illustrating the applications of geology to the useful purposes of life" is a desideratum, and we do not for one moment deny that it is, then let us have one, and let it be in the place that is most convenient to engineers, architects, well-sinkers, medical officers, and such practical men. But all this has mighty little to do with the fine, stratigraphical series of British fossils, of which only a small portion is named and exhibited at Jermyn Street, and that in a manner to reflect credit on the palaeontologists of a past generation.

French 'Protection' of Fossils in Madagascar

We learn from the July *Geographical Journal* that the *Politique Coloniale* of May 25, 1898, publishes a circular issued by the French Governor of Madagascar, ordering the local officials in this colony to prevent any but Frenchmen from collecting fossils in the island. No one is to be allowed to collect fossils unless he be provided with a special licence from the Governor; and this will only

be granted to his fellow-countrymen. Further than this, complaint is actually made that foreign scientific men have already secured too many of the fossil treasures of the island. We wonder whether the naturalists of France, official and otherwise, have been consulted on this subject, or whether it is merely the order of a politician ignorant of the methods of scientific men. It will, indeed, be strange if the enlightened Government of France, which does so much for the promotion of research in foreign lands, should allow this policy to be pursued in its latest dependency. Natural science has hitherto known no division into nationalities. On the contrary scientific work confers a free-masonry on those who pursue it, and is the strongest force towards the federation of the world. It should not be turned into a cause of division. It is not long since the Colonial Government of Mauritius paid for excavations in that island to exhume the fossil remains of birds. These were investigated and described by our own ornithologists in the University of Cambridge, and were faithfully returned by them to the President of the Excavation Committee for preservation in the Museum of the Colony. Among other treasures then obtained was the finest known skeleton of the Dodo. This, however, along with other specimens, was eventually removed from Mauritius to the Museum of Natural History in Paris, where it is now one of the greatest ornaments. Surely a nation which can accept foreign courtesy in such a manner can ill afford to countenance such petty spite as that displayed in the manifesto of the Governor of Madagascar. We trust it is enough to draw the attention of French naturalists to the subject, that they may use their influence in a matter which ought to be beyond all political considerations.

An American Pirate

In our June number we published a specially written article, entitled "A Geographical Commemoration: Vespucci, Deschnev, and Vasco da Gama." This article has been reprinted in the *Scientific American* for July 2, 1898 (vol. lxxix. p. 8). A few verbal alterations have been made, causing the article to appear as though prepared by the staff of the *Scientific American*. Five lines in the article are, it is true, placed between quote-marks, and ascribed to *Natural Science;* but no one would imagine from this that the whole article was lifted bodily from our pages. What makes this treatment worse is, that we acknowledged quite fairly and frankly that some of our statements concerning Vespucci were taken from an account in our contemporary. This is not the first time we have had to complain of similar piracy on the part of the *Scientific American*.

Scenery and the Poets

The Romanes Lecture for 1898 was delivered in the Sheldonian Theatre at Oxford on June 1, by Sir Archibald Geikie, who took for his subject "Types of Scenery and their influence on Literature." This has now been published, at the price of two shillings, by Messrs Macmillan & Co. The subject of the lecture is less than its title. Scenery is limited to Great Britain; its types are defined as Lowlands, Uplands, and Highlands; and by 'literature' we are for the nonce to understand 'poetry.' Even within these somewhat narrow bounds, we are not sure that Sir Archibald has made the most of his theme. It was a delightful lecture, full of pretty word-painting and apt quotation, with a geological flavour deftly introduced so as to make the listener think that there was a foundation of abstruse science, and that this particular branch of science was perfectly charming. But as an essay, as a contribution to serious thought on the subject, its tenuity approaches transparency. A poet, we gather from the lecture, describes what he sees, and draws his images from his surroundings. Cowper depicts the valley of the Ouse; Thomson turns from "the living stream, the airy mountain, and the hanging rock" of his native Border to Hagley Park and the "sleep-soothing groves" of the south of England; Burns sings the "banks and braes o' bonnie Doon" and "the bonnie banks of Ayr"; Wordsworth, fortunately for English literature, lived in the highlands of the Lake District, and introduced to us mountains and lakes and sounding cataracts, "clouds, mists, streams, watery rocks and emerald turf." But all this, though we may not have heard it expressed in such felicitous language, we knew before.

The most striking and original passage in the essay is that which applies the scenic method of criticism to Macpherson's "Ossian." "The landscape," we are told, "belongs unmistakably to Western Argyleshire. Its union of mountain, glen, and sea removes it at once from the interior to the coast. Even if it had been more or less inaccurately drawn, its prominence and consistency all through the poems would have been remarkable in the productions of a lad of four-and-twenty, who had spent his youth in the inland region of Badenoch, where the scenery is of another kind." "It is not that in Ossian, highland landscape was deliberately described, but it formed a continually visible and changing background. The prevalent character of the whole range of scenery in the region, and the general impression made by it on the eye and mind, were so vividly conveyed that no one familiar with the country can fail to recognise how faithfully the innermost spirit of the West Highlands is rendered." This is strong as well as new evidence in favour of the authenticity of at least a large proportion

of the Ossianic poems. Moreover, as to the question whether the English or Gaelic is the original, Sir Archibald says, "There can be no doubt that on the whole the Gaelic is more vivid and accurate in the description of landscape than the more vague and bombastic English of Macpherson."

In Ossian we are dealing with true nature-poetry; equally unsophisticated are the Border ballads, and it is in the essential character of such folk-poems rather than in the allusions, descriptions, and similes of more cultured and artificial poets, that search should first be made for the influence of scenery and topography upon literature. If scenery have any influence upon the subject-matter, the form, or the niceties of style, that influence will be discovered more readily by comparing groups of writers from different regions (such as the Lake School, the Attic Dramatists, the Norwegian Novelists) and estimating their common characters, rather than by contrasting individual writers. There are more important subjects that call for investigation, and yet we should not be sorry if Sir Archibald Geikie's eloquent lecture led to further essays on these lines. Literature to-day has so largely become a matter of "words, words, words," that the mere suggestion of a possible connection with external nature cannot fail to do good. And if our stylists can be brought to look at the larger conception of nature resulting from modern science, or if our scientific students can be led to look on literary form with less contempt, then it is possible that the literature of the future may share in the remarkable progress that has already fallen to the lot of modern science.

Models of Multipolar Cells

Professor A. L. Herrera, to whose curious experiments on the production of artificial simulations of organic structures we referred in February last (vol. xii., p. 74), has sent to us from the National Museum at Mexico an account of a new result he has obtained. He noticed accidentally that when a greasy solid is lightly dabbed with a brush dipped in a viscous liquid, the liquid rapidly assumes the form of a network of multipolar ganglion cells. He sent us along with the letter a shallow tin box, the bottom of which, on its inner surface had been greased with butter and then had received an application of some coloured viscous fluid. This fluid had assumed the configuration of a group of multipolar cells, and when it reached us, still retained that appearance. Dr Herrera wishes to correlate this observation with the older experiments upon the artificial production of nervous simulacra out of myelin, as described in Robin's treatise on the microscope (Paris, 1871, p. 569). We are not prepared to go so far as the Professor in believing that such experiments throw light upon histogenesis, but they are interesting and ingenious.

Synthetic Protoplasm

Professor Herrera also sent us a letter containing an account of some experiments he has made on what he calls a 'synthetic protoplasm' made by him by mixing pepsine, peptone, acetic fibrine, oleic acid, soap, sugar, extract of bile, carbonate of soda, lime, and of ammonia, lactate of lime, phosphates of lime and magnesium, sulphates of lime and iron, and chloride of sodium. When this compound is placed in water and examined under the microscope, violent diffusion currents are set up, and streaming movements of a very active kind last for a few minutes. When this is over, the addition of a trace of liquid ammonia renews the activity, which lasts for several hours. These movements he rightly attributes to the liberation of gases by the particles of the compound, and he compares with this the liberation of carbonic oxide by living protoplasm, suggesting that in the discharge of that gas lies the secret of the streaming movements of protoplasm. Professor Herrera was good enough to send us a small quantity of his compound, and on this we have successfully repeated some of the experiments he describes. The little mass was particularly active in water with a trace of ammonia: diffusion currents, movements of the whole mass, and the protrusion of pseudopodia-like processes occurred. We found, however, that the resemblance to protoplasm was destroyed from the fact that the mass did not retain its coherence; small masses were constantly discharged from its surface, and it seemed to melt away in a comparatively short time. In this respect it compared unfavourably with Bütschli's foams, and we imagine that, although Dr Herrera has imitated with considerable success the very complicated chemical composition and consequent instability of protoplasm, he has not been so successful with its structure. On the other hand, it is possible that the substance, in its postal journey from Mexico, has deteriorated. We think that he has begun a most ingenious line of experiment, and wish him all success in his further attempts.

The Phyto-Plankton of the Atlantic

On May 12, Mr George Murray and Mr V. H. Blackman presented to the Royal Society their observations on a year's work in collecting phyto-plankton along a track from the Channel to Panama, carried out by Captains Milner and Rudge, and also during one voyage to Brazil by Captain Tindall. They also gave the results of their own observations on living material at sea. The material was obtained by the pumping method described in our June number (vol. xii., p. 367).

One of the objects of the authors' work was to determine, if possible, the nature of the microscopic and little understood objects known as coccospheres and rhabdospheres. In the present paper

they describe the minute structure of the calcareous plates or coccoliths and rhabdoliths, and record the existence in the coccospheres of a single central green chromatophore, separating into two on the division of the cell. They regard Coccosphaeraceae as a group of Unicellular Algae, and they define the group with the limits of its genera and species. The coccospheres and rhabdospheres from the surface are compared with those of the deep-sea deposits and their identity established. They are also compared with geological coccoliths and rhabdoliths from various beds, and many objects regarded by geologists as true coccoliths and rhabdoliths are rejected.

A large number of new Peridiniaceae were discovered and are formally described and figured. No specific diagnoses of marine Peridiniaceae have previously been published, authors of species having depended on figures, and, at most, a few words of description. No doubt the present systematic treatment of the subject will conduce to greater order in the group. The authors record the occurrence of all the forms in seven tabular statements, one for each collecting voyage.

A study was also made of the species of *Pyrocystis*, of which a new one is described. The facts here recorded tend, in the opinion of the authors, to confirm the view originally expressed by Sir John Murray, its describer, that it is an unicellular alga, though doubts had been entertained of the accuracy of this opinion by several biologists.

The Missouri Garden

The ninth annual report of the Missouri Botanical Garden, issued by the director, Professor Trelease, has just reached us. As in the case of previous issues, it contains the garden reports, embellished with photographic reproductions of features of interest, and a number of scientific papers, several of which deal with those dry-loving tropical plants for the growth of which the garden is eminently suited. One of these papers, a revision of the genus *Capsicum* (which includes the Chili and Cayenne peppers), with special reference to garden varieties, by H. C. Irish, studies an interesting example of the assumption of numerous forms by plants which have been long cultivated. The genus, which is an ally of *Solanum*, the potato and tomato genus, evidently originated in the tropics of America, whence it was probably first brought to Europe by Columbus, and is now widely spread in the Old World tropics. Linnaeus made four species, and the number of specific names has since so increased as to reach at the present time nearly one hundred, of which the Index Kewensis recognises over fifty as good. Asa Gray, however, suggested that these might perhaps all be reduced to two, and Dr Sturtevant, who made a special study of the genus, collecting and

cultivating a great many kinds, also came to the conclusion that a majority of the published species were merely varietal forms, and that the number which botanists would hold as good species would be very small. Dr Sturtevant recently gave the whole of his material, including herbarium-specimens, drawings, and notes to the Missouri Garden on condition of the ultimate publication of the results of further study in the form of a monograph. This has now been done by Mr Irish, who has confirmed Dr Gray's suggestion as to the existence of but two species. One, *Capsicum annuum*, is herbaceous or slightly shrubby, and of annual or biennial duration; the other, *C. frutescens*, is a shrubby perennial. The former is responsible for the great majority of the cultivated forms, which are distinguished by characters based mainly on the shape, size, and erect or pendent position of this fruit, and in the form of the calyx. Many of the forms are depicted in the twenty-one plates which accompany the monograph.

Among the other papers are a revision of the American duckweeds (*Lemnaceae*) by C. H. Thompson, and notes on a willow, *Salix longipes*, from the South-Eastern States, by Dr Glatfelter, both of which have been previously issued. There are also notes on species of *Agave*, *Cactus* and *Yucca* by various authors, and a list of Cryptogams collected by Mr A. S. Hitchcock in the Bahamas, Jamaica, and Grand Cayman. Among the last mentioned plants Mr J. B. Ellis finds fourteen species of fungi belonging to the group Pyrenomycetes, of which nine are new. A fungoid disease which had attacked the leaves of certain palms belonging to the same genus as the date (*Phoenix*) is described by Professor Saccardo as caused by a new species.

In addition to the five plates which refer to the Garden, there are fifty to illustrate the various scientific papers.

Wasted Wealth

It is often the case, even in these days of technical education, that for want of a little elementary knowledge the wealth which lies at our feet is entirely overlooked. A remarkable instance of this is furnished by the chemical manure factories of Lincoln. These supply a large proportion of the artificial manures used in the kingdom, yet the whole of these commodities are manufactured out of imported material. A well-informed writer in the *Lincolnshire Chronicle* has lately pointed out that all the expenses of importation might be saved.

"In view of the great stores of mineral phosphates that lie in the rocks on which Lincoln city is built, it is," he says, "inexplicable that our factors should go to the trouble and expense of bringing in all this foreign material. For those interested in this subject a visit to Handley's Brickyard, just below Swan's Pit, will prove a valuable object lesson. Interstratified with what are known as the Marlstone

and the Shale, lie several seams of so-called coprolites or nodules of phosphates of lime. The seams vary in thickness from three inches to eighteen inches, and all are formed of these nodules closely compacted. We have had an opportunity of assisting at an analysis of these nodules, from which it was demonstrated that not only are they remarkably free from iron, but that they are ten per cent. richer than are the imported Belgian samples. It must not be supposed that these seams are peculiar to Handley's Pit only—they extend uniformly throughout the middle hills of Lincolnshire, and are exposed in most of the pits from Lincoln to Grantham. If properly exploited the phosphate industry in Lincolnshire might be made second only to that of its great iron mines and foundries."

Humber Mud

THERE is mud in the Humber estuary, mud and sand, and a great deal of it. Where does it come from?

This is the question that the Hull Scientific and Field Naturalists' Club set itself to discuss on April 13th, the discussion being opened by its energetic secretary, Mr T. Sheppard. The usual reply that the mud comes from the tributary rivers, Ouse, Trent, and Hull, is soon found to be inadequate. The mud is accumulating so rapidly that the material brought down by the rivers is insufficient to account for it. Not only sand and mud banks but dry land is continually being formed. Reed's Island, between North and South Ferriby, was, some twenty-five years ago, comparatively small, with a plot of grass on which a few cattle were reared. Now it is hundreds of acres in extent, has an enormous number of cattle grazing upon it, and is still growing. As for the accused rivers, their waters are comparatively clear and hold but few particles in suspension. Such detritus as they do transport is for the most part redeposited on their own banks, or in the alluvial flats so characteristic of the Ouse and Trent. It is, however, noticed that the water near the mouths of these rivers is far more muddy when the tide is flowing up them; and this suggests that the mud may be brought into the Humber from the sea.

The waters of the North Sea are continually washing particles of rock, sand, and mud in a southerly direction, and slowly but surely the material on the Yorkshire coast is travelling southward. It never travels in a northerly direction. As is well known, the beach around Flambro Headland is strewn with masses of chalk of all sizes, which have been dislodged from the cliffs. These can be seen in plenty in Bridlington Bay and further south, though naturally getting less plentiful as they get Humber-wards. Hardly any chalk boulders are found north of the headland. This proves that the beach-material travels southward. Now the cliffs of the Holder-

ness coast are made up of soft glacial clays, capped in one or two places by lacustrine deposits of small extent. They vary in height from 10 to 50 feet, and at Dimlington reach over 100 feet. Mr J. R. Boyle has shown on historical evidence, and the Rev. E. M. Cole and others have proved by direct observation, that the whole of the cliffs from Bridlington to Spurn are being eroded at an average rate of about 7 feet per annum. The whole of the eroded material must be gradually, or in some cases quickly, converted into gravel, sand, and mud, and carried southwards. A large quantity of this material is carried past the Humber mouth and is gradually silting up in the Wash and off the Lincolnshire coast. At the same time a deal of it must be brought into the Humber at each tide; and when the winds are the strongest, and the erosion most severe, the inrush of water into the Humber is likewise the greatest. This water brings with it the cliffs in a modified form. It would appear, therefore, that it is from the coast that the bulk of the material suspended in Humber waters is derived.

It does not follow that the mud now in suspension in the Humber is the result of one or two tides. The particles may have been accumulating during several months, and undoubtedly pass and repass a particular point several times a week. Consequently, when the rivers flowing into the Humber are swollen with flood waters, and are swift, the muddiness observed near their entrances to the estuary is not necessarily due entirely to the additional material which they have brought down, but is more likely to be owing to the sediment in the Humber being stirred up.

Absolute confirmation of this theory as to the origin of the Humber mud, such as might under other circumstances be afforded by microscopic examination of the mud-particles, is not to be obtained, since the particles brought down by the rivers are precisely similar to those found in the cliffs of the east coast. Not only are the boulders in those cliffs formed of rocks similar to those eroded by the rivers near their sources; but in their lower reaches the rivers traverse boulder-clay areas.

Slugs

There are before us some papers dealing with slugs, and written by Mr Walter E. Collinge, one of the few British workers who have turned their attention, with any persistency, to this branch of malacology.

The forms included among slugs are not of necessity near relations, nor are they, as some might imagine, sharply separated off from types with well-developed shells. Nevertheless, slugs as a whole present the same difficulties, and require to be approached from the same point of view.

The student of slugs obviously cannot be a mere conchologist, but one of two temptations may beset him according as he is or is not an anatomist. In the latter case, he may be led to attach undue importance in descriptive work to colour variations, occasionally making one species out of two, but more often splitting up one into several, and possibly naming 'varieties,' which may be based merely upon such phases of coloration as succeed one another during the life-history of a single individual. The anatomist, on the other hand, may be inclined to consider as diagnostic of species points of internal difference so small as to suggest that the describer's wish was father to his thought. And this is especially the case when little or no outward differentiation is obvious. Of course, in these days when 'physiological species' are recognised, there is nothing inherently improbable in the assumption that there are forms alike without yet not within, provided that some additional evidence is forthcoming as to varying habits or development. It seems reasonable, however, to expect something of the latter sort to be brought forward before we are asked to recognise 'anatomical species' as good ones.

Mr Collinge is an anatomist, and in his paper "On some European Slugs of the genus *Arion*" (*Proc. Zool. Soc.*, 1897, pp. 439-450, pls. xxix.-xxxi.) he brings forward evidence as to "the constancy of anatomical characters" so far as the genital organs are concerned, these being the parts in which specific differences are mainly sought. The testimony is based upon the small number of variations found in a large series of specimens dissected, and belonging to the same two species of *Arion*. That the form of generative organs in the particular genus considered presents but little modification throughout the species is a well-known fact, and, even if this were not so, a comparison between fig. 3 of *A. subfuscus* and fig. 12 of *A. hortensis* would suffice to show it. To bring forward this 'constancy' in *Arion* as militating against Cockerell and Larkin's belief in the specific identity of several forms of *Veronicella*, which differ anatomically, is but lost labour. So far as *Arion* itself is concerned, the small number of slight variations noted by Mr Collinge (*i.e.*, 26 out of 1223) may serve as an excuse for increasing the number of species when more marked differences are from time to time detected.

Mr Collinge, indeed, in the paper under discussion, reserves his *Arion hortensis*, var. *caeruleus*, which differs more markedly from the typical *hortensis*, and raises it to specific rank. We note that *A. hortensis* is figured with but one vestibule, while *caeruleus* has two: two, however, are shown in an unpublished drawing of the former by the late Charles Ashford now before us, while Mr Collinge's fig. 12 contradicts his own statement, that in no species of

the *A. hortensis* group is the lower portion of the free oviduct much larger and more globose than the upper. Again, in the paper, the validity of *A. fuscus*, externally very much like *A. subfuscus*, is admitted by Mr Collinge who previously rejected it. This is not to be wondered at since, in another paper on four species of the same genus, published about the same time in the *Journal of Malacology* (vol. vi., pp. 7-10, pl. ii.), almost the only evidence of external difference between *A. empiricorum*, *A. ater*, and *A. lusitanicus* given by Mr Collinge is a short series of measurements pointing to the fact that the second species is larger than the first.

Of course it is probable that our islands contain more species of *Arion* than many of us used to imagine, and Mr Collinge has done much to arouse us to the truth about them; but, before all the species which he and his continental friends would give us are recognised by thinking malacologists, still more careful and detailed work must be done upon the genus. We have been so busily criticising the weak points in the paper that we have only hinted, by quoting a few figures, at the prodigious amount of work which it must have entailed. There only remains for us to express the hope that other workers will come forward and supplement what is a valuable contribution, from an anatomical point of view, to our knowledge of the genus *Arion*.

In the other paper we have "the description of two new species of slugs of the genus *Parmarion* from Borneo" (*Proc. Zool. Soc.*, 1897, pp. 778-781, pl. xliv.). One of these the author considers intermediate between the genus in which he places it, and *Microparmarion*; in fact it leads Mr Collinge to the conclusion that no line can rightly be drawn between the two genera.

American Isopods

Mr J. E. Benedict on March 24 published a paper on "The Arcturidae in the U.S. National Museum" (*Proc. Biol. Soc. Washington*, vol. xii., pp. 41-51), in which there are several interesting features. He describes eight new species, and of each of them gives a good and intelligible 'habitus-figure,' an adjunct without which the most lucid description of a new form is usually difficult and wearisome reading. Within the last twelve years the number of species in the genus *Arcturus* has been raised from five to twenty-five. Mr Benedict has therefore rendered a kindly service by supplying a key to this rapidly increasing group. As often happens, when new species appear, some of the old generic distinctions have to disappear. Mr Benedict finds, for example, that in some species of *Arcturus* the fingers are not simple, but biungulate as they are in *Astacilla*. It appears, too, from his descriptions that *Arcturus* cannot invariably be distinguished from

Astacilla by having the flagellum of the second antennae more than four-jointed. Nature lures us on by showing species after species in which the joints are more than four, and, when we are purely satisfied with our generic character, brings to view *Arcturus multispinis*, in which the flagellum in question is single-jointed. When *Astacilla* has established a character for preferring shallow water, Mr Jules Bonnier describes the remarkable *Astacilla giardi* from a depth of more than 500 fathoms, and now from 1825 fathoms comes Mr Benedict's *A. caeca*. Mr Beddard has pointed out that the genera *Arcturus* and *Astacilla* "form almost the only exception to the general statement that the deep sea Isopoda are blind," and now Mr Benedict's last-mentioned species comes as an exception to this exception, being a blind Isopod from the deep sea.

In a second paper, simultaneously published in the same *Proceedings* (vol. xii., pp. 53-55), Mr Benedict describes two new Californian species, both of which he assigns to the genus *Idotea*, relying for the limits of that genus on the monograph of the Idoteidae by E. J. Miers, published in 1883. It is rather surprising that he takes no notice of the far more recent discussion of this family by Mr Adrien Dollfus in the *Feuille des Jeunes Naturalistes*, November 1894, and February 1895. Few students who have read the papers by Mr Dollfus will be ready to retain Mr Benedict's two new species in one and the same genus.

Miss Harriet Richardson, also in the same publication (xii., pp. 39-40) describes and figures a new species of *Æga*, closely related to *Æga tridens*, Leach. The discriminating characters do not seem to be all of them quite convincing. One of these depends on the number of joints in the flagella of the antennae, the new *Æga carinata* having on the second pair ten joints, while Leach's species has nineteen. It is true that the number nineteen is assigned to it by Schiödte and Meinert, but Bate and Westwood say that the number is about twelve, and it may be taken for granted that there is no fixity in this respect to depend on among specimens of different ages and different sizes. The relative length and breadth of the body is likely to prove an equally unstable character. The new species is said to be more than thrice as long as broad, and the figure given agrees with this measurement, but so does the figure of *Æga tridens* given by Schiödte and Meinert, though that drawn by Bate and Westwood is of a more portly habit.

Under present conditions of human existence the scattering of scientific information remains unavoidable, so that one can merely note, without astonishment or disapproval, that one new species of *Æga* obtained by the U.S. Fish Commission steamer 'Albatross'

is described by Miss Richardson in the *Proceedings of the Biological Society of Washington*, and that four new species of the same genus obtained by the same vessel are described almost at the same time by Dr H. J. Hansen in the *Bulletin of the Museum of Comparative Zoology* at Harvard College. Of the high merit of all Dr Hansen's zoological work mention has been too recently made to need further comment on this occasion. In regard to Miss Richardson's excellent contributions to our knowledge of the Isopoda, the suggestion may be diffidently hazarded that researches reported in a collected and connected form are now-a-days more acceptable than isolated descriptions.

Some Mexican Birds

In a short paper published in the *Proceedings of the Biological Society of Washington* (vol. xii., pp. 57-68; March 24, 1898) Mr E. W. Nelson includes a critical examination of the long-nailed partridges, for which Mr Ogilvie-Grant established the genus *Dactylortyx* in 1893 (cf. Cat. Brit. Mus. xxii. p. 429). Mr Ogilvie Grant was only able to include a single species, *D. thoracicus*, under this genus; but Mr Nelson decides that Mr Ogilvie Grant united two distinct species, of which he supplies the distinguishing characters. He also describes two new species of long-nailed partridges, both obtained in Mexico. Mr Nelson has discovered several other species and sub-species of birds in Mexico. Of these, perhaps the most surprising novelty is the Sinaloa Martin (*Progne sinaloae*), procured upon the western slope of the Sierra Madre, between 2500 and 4000 feet altitude. Oddly enough, this new species from North-west Mexico is closely related to the pretty Caribbean Martin (*Progne dominicensis*) which is peculiar to the West Indian Isles. A good figure of the latter will be found in Sharpe and Wyatt's "Monograph of the Hirundinidae," vol. ii. plate 91.

The Black Kite

Count Arrigoni Degli Oddi is one of the most enthusiastic of the younger generation of Italian ornithologists, and has recently published several excellent papers on the birds of his country. An essay just issued by him, at Venice purports to be a notice of the nesting of *Milvus migrans* in the province of Verona, but it is, in point of fact, almost a life-history of the Black Kite. The author supplies dates for the arrival and departure of this hawk in and from Verona for a term of fifteen years, from which we learn that it reaches its summer quarters in March, and leaves for Africa again in August, or at the commencement of September. The duties of incubation are performed by the female bird, and occupy from eighteen to twenty days. The old kites are devoted to their

young ones, which, we learn with regret, are fed largely upon small chickens. But, lest it be supposed that this kite should be banned as vermin, we hasten to observe that its diet is very varied, including worms, small crustaceans, dragon-flies, grasshoppers and other insects, frogs, toads and water-newts, lizards, snakes of two species, and five species of fish; not to mention small mammals, such as moles, shrews, mice, and (last, but not least) land-voles, which are the bane of agriculturists.

An Alga parasitic on Ophiurids

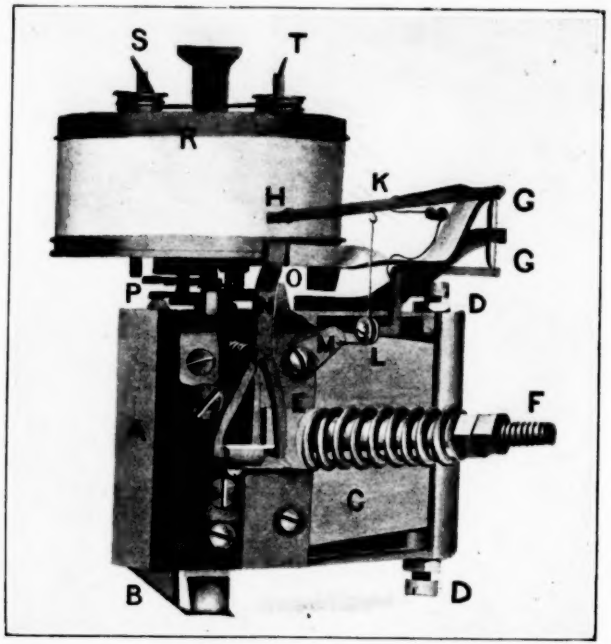
The occurrence of Algae in intimate connection with the bodies of living animals is well known. Such are the yellow cells (*Zooxanthella*) of radiolarians, the green alga, *Zoochlorella*, found in many infusorians, in *Spongilla*, *Hydra*, in various rhabdocoele turbellarian worms, and so forth. But in all these cases the relation is one of symbiosis, on the give and take principle. Lately, however, Dr Th. Mortensen has described in *Videnskabelige Meddelelser* (1897, pp. 322-324) a species (probably a new one) of the green alga *Dactylococcus* infesting the brittle-stars *Ophioglypha texturata* and *O. albida*. It forms dark-green patches on both disc and arms, on both upper and under side, sometimes in small excavations in the calcareous plates. Its peculiarity is that it eats away the skeletal substance and, apparently, does not pay for it. It is a true parasite.

Paper

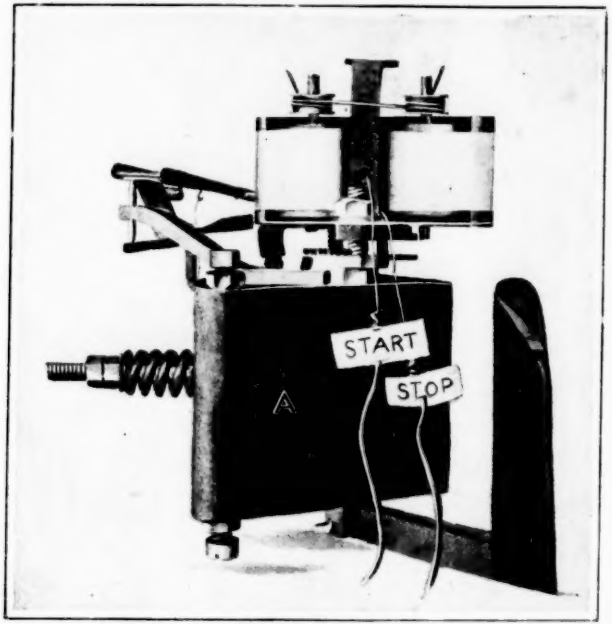
Since every scientific man should believe that his work is of permanent value, and should avoid publishing anything in which he does not so believe, it becomes of some interest to him to see that the paper upon which his articles are printed is of a quality warranted to last. Therefore we recommend our readers to consider with care the valuable report printed in the *Journal* of the Society of Arts for May 20, in which a Committee appointed for the special purpose, give the results of their investigation into numerous printing papers. The disintegration of the fibre of papers is, as a rule, referable to acidity in the case of rag papers: while, in those made from wood-pulp it is due to oxidation, and is accompanied by an alkaline reaction. The discoloration of papers is proportional to the amount of rosin which they contain. The Committee, therefore, advocates the following specification as that of a reliable paper :—"Fibres: not less than 70 per cent. of fibres of the cotton, flax, and hemp class. Sizing: not more than 2 per cent. rosin, and finished with the normal acidity of pure alum. Loading: not more than 10 per cent. total mineral matter (ash)."

UNIV. OF RICH.

General View.



Back View.



CONTINUOUS RECORD INDICATOR, *with cover removed, to show Pencil Attachment and Drum.* *A*, Front Plate; *B*, Bottom of Axle; *C*, Link; *D*, Dead Centres for Link; *E*, Plate End of Spring; *F*, Core and Link Support of Spring; *G*, Vertical Axis of Pencil Attachment, but Horizontal Axis just above; *H*, Pencil; *K*, *L*, *M*, Chain; *L*, Pulley; *N*, Winding Lever; *O*, Winding Wheel to left of this; *P*, Cam for putting Pencil out of action; *R*, Drum on which diagram is drawn; *S*, *T*, Cylinders holding paper.

I

Some More Rowing Experiments

IN the autumn of 1895 the author designed an indicator for recording the work done in a stroke of rowing.[1] The apparatus replaced the front thowl of the rowlock, and consisted of a plate turning with the oar, which pressed against it, about an axle to which it was connected by means of another plate pivoted to the first.

The pressure of the oar tended to move the front plate back towards the axle, this motion being resisted by a spring. The movements of the plate, and consequently of the oar, were recorded as a diagram on a horizontal card by a pencil connected with the plate. This diagram afforded the information given by the familiar steam-engine indicator diagram, and gave measures of the horse-power of oarsmen, as well as interesting information with regard to the way in which the work was done, showing the great differences that existed between the style and stroke-forms of different oarsmen.

The successful working of this simple instrument encouraged the author in 1896 to attempt another, obviating the shortcomings of the first. This latter, in addition to various mechanical imperfections, had the disadvantage of giving the diagram in curvilinear co-ordinates, necessitating laborious measurement and reduction before stroke form and work done could be estimated.

Further, in testing a steam-engine it is customary to take several diagrams during a run, by changing the card on which the figures are being drawn. In a boat this is impracticable without stopping the rowing. It will be seen that an important part of the later machine is the automatic winding apparatus, whereby the card changes itself. The author was recently interested in having his attention called to a similar device which was being introduced for the steam engine.

The general principle of the former indicator, that of recording the movements of a plate turning with the oar and pressed forward by a spring, has been adhered to. In the photographs (Plate I.) and diagram illustrating the action of the pencil and spring (Fig. 1) the face *A* moves with the oar, and is connected with the axle *B*

[1] For a description of the instrument and results obtained, see *Natural Science*, vol. viii., pp. 178-185, March 1896.

(which screws on to the rowlock in place of the thowl) by means of the link *C*. This junction piece, which is strongly braced to avoid flexure, rotates round the axle in an adjustable bearing, and is pivoted to the face at *D* in dead centres. The spring is placed between *E* on the back part of the front plate, and a nut at *F* on a core, fixed at *f* to the link.

The part of the instrument in which it differs essentially from the earlier one is the recording mechanism, in which the diagram is obtained practically in rectangular co-ordinates, and every fifth stroke is automatically indicated for a period up to 500 strokes.

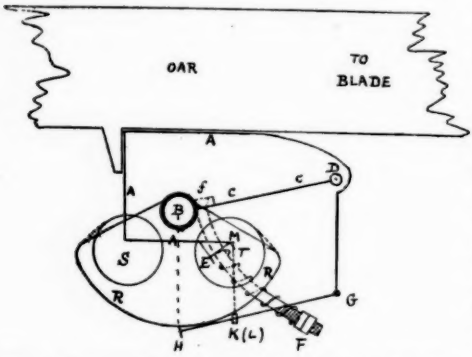


Fig. 1. Diagram illustrating action of Pencil and Spring.

The diagram is recorded on a drum (*R*) having its axis along the axle. The strip of 'metallic paper' on which the diagram is drawn winds off the cylinder *S*, over the drum on to the cylinder *T*, and is held in position by an elastic thread which winds off a pulley on *T* on to a similar pulley on *S*.

The pencil attachment (*HGL*) moves as a whole about a vertical axis *G* fixed to the face, while the arm *GH* can move about a horizontal axis fixed to the main attachment, which keeps it in a vertical plane with the pulley *L*. *G* is so placed that (Fig. 1) *BDGH* is a parallelogram (assuming for the moment that *H* has no vertical movement), and, consequently, if *H* remains on the drum while the face moves parallel to itself towards the axle, it will stop in the same position. The pencil (*H*) (a brass point) is pressed upwards by a spring, and is held down by a chain which passes over the pulley *L* and is fixed to the face at *M*.

If the front face moves backwards parallel to itself, *G* and *M* participate in the movement, while *H* rests on the drum owing to the spring, which, keeping the chain tight, pulls *L* towards *M*. Consequently *M* approaches *L*, and some of the chain passes to the vertical part, and allows the pencil to move upwards. In this way a pressure line is drawn, which is a circle about *G* as centre. The maximum error introduced into the position of the oar by regarding this as a straight line is about 0·5°,[1] while the line of no pressure

$$^{1} = 3\left(1 - \cos\sin^{-1}\tfrac{1}{2}\tfrac{3}{4}\right) \times \frac{360}{2\pi \times 2}.$$

produced by simply turning the oar, is a horizontal circle, which becomes a straight line when the diagram strip is unwrapped.

At the conclusion of a stroke, when the pencil has reached its extreme position to the right, the oar leaves the face and rests against the opposite thowl, only touching the instrument at the rounded end of the face to carry it into position for the next stroke. During this movement the pencil travels over the base line, and a lever *N* engages a spoked wheel *O*, which gears into the right hand cylinder and winds the strip into a new position. As the winding ceases, a cam-wheel *P* is rotated; this drives a plate attached to the bottom of the drum outwards, and thus lifts the pencil off the strip and the winding lever out of gear.

During the stroke the wheels are not engaged, since the levers share the backward movement of the face when the spring is compressed and pass clear of the wheels. While the cam holds the plate out no diagrams are drawn and no winding goes on. Each forward swing carries the cam wheel forward one tooth, until at the fifth stroke the pencil again reaches the paper, another diagram is drawn and the strip is once more wound. Two cords place the starting and stopping of the record under the control of the coxswain. The upper part of the instrument is protected from splashing or rain by a cover.

Knowing the diameter of the drum, and the strength of the spring, the diagram gives a result in pounds pressure per degree of turn of the oar. In order to express the result in foot-pounds of work, the ratio of the pull on the handle of the oar to the corresponding pressure on the rowlock must be known, and the distance through which the handle moves per degree of turn. These two constants are found from the in and outboard dimensions of the oar, and the position of the centre of pressure of the water on the blade.[1] The spring was calibrated in the Millard Laboratory, Oxford, by kind permission of Rev. F. J. Jervis-Smith. With the oar and spring used in the experiments, a height on the diagram of 1″ represents a pull of 220 pounds, and 1 sq. inch represents 377 foot-pounds.[2]

When the indicator is fully open it is usually convenient to have the spring under some initial compression. This is estimated by noting the turns given to the nut *F* after it has just touched the spring, and allowed for by measuring heights from a new base-line 0″·06 per turn below the actual one. Experiments in calibrating the spring showed that one end did not completely 'bed' itself until some pressure was exerted. The error due to this is practically eliminated by raising the base line 0″·03.

[1] Neglecting the moment of inertia of the oar about its button. See Appendix I.
[2] See Appendix I.

In order to prevent the necessary length of the strip being excessive, the diagrams are made to overlap (Fig. 2). If the strokes are flat at the top it is frequently not easy to determine at sight which is the 'finish' corresponding to a particular 'beginning.' This is, however, discovered at once by measuring the length of the first or last stroke, from which the approximate distance of the 'finish' from any 'beginning' is known. A stroke thus individualised can then be measured up. The measurements taken are:—

(1) Length of stroke in degrees: which is afterwards reduced to distance moved through by the handle of the oar.

(2) Greatest height of diagram.

(3) Area, measured with an Amsler planimeter.

(4) The shape of the curve gives the style of stroke.

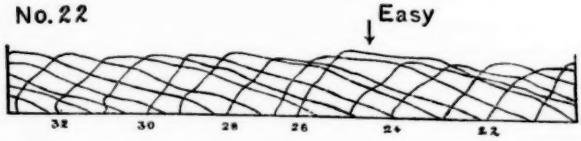


Fig. 2. Part of an Indicator Card (two-thirds actual size).

To determine the style of an oarsman under any particular circumstances, the idiosyncrasies of the strokes are eliminated by superposing several diagrams (Fig. 3), and then drawing a mean line through the result. In this way the 'Characteristic Diagram' is obtained. Similarly, in finding an oarsman's power, the mean of several stroke measures is taken.

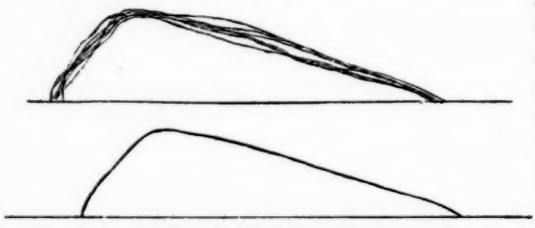


Fig. 3. Deduction of Characteristic Diagram (actual size).

Having now described the indicator, the diagram, and the method of treating it, some account will be given of the results obtained from twenty-seven experiments involving some 2000 strokes of rowing. The principal oarsmen experimented on are denoted by *A*, *B*, *C*, *D*, *E*, *F*, *G*, *H*, *I*.

Fig. 6 represents a series of characteristic diagrams of rowing on sliding seats. *K* was obtained with the earlier indicator, but is introduced here as the stroke-form differs from any others in no small degree. This stroke although very short is nevertheless very powerful.

The horizontal dotted line corresponds to a pull of 100 pounds and the vertical one to a stroke length of 80°.

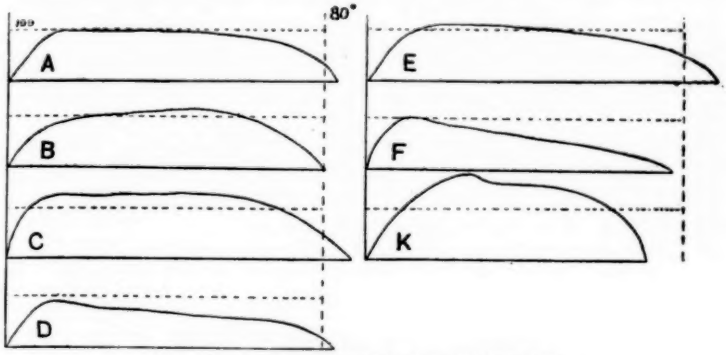


Fig. 4. Some Characteristics—Sliding Seats.

Four fixed-seat characteristics are shown in Fig. 5. The results given in the table are generally deduced from a larger number of measures than the diagrams, and in some cases additional experiments have been introduced, so that the correspondence between the figures and table is not necessarily exact.

An inspection of the characteristics reveals the great individuality in stroke-form that exists even among men who have rowed together and undergone the same course of instruction, and using the same boat and oar. Two diagrams could scarcely be more different than *B* and *F*. The author has found that the form *B* is rather typical of the heavy man's stroke—a powerful stroke, but with a sluggish beginning, while *F* typifies the light man who has a smart beginning, and quickly reaches his highest pressure, which he lacks strength to continue through the stroke.

A comparison of the sliding seat with fixed seat strokes, shows that the latter generally have a much weaker finish, suggesting that the chance of good leg-work at the finish is diminished on a fixed

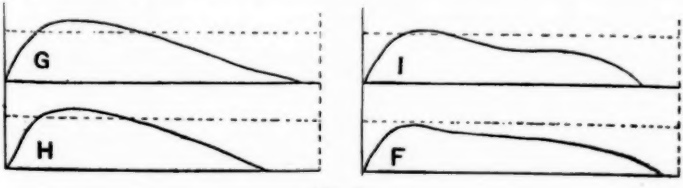


Fig. 5.

seat. As is to be expected, the strokes are some 8 inches shorter. This shows—as an oarsman well knows—that a 14″ slide does not add its full length to the stroke, since the 'swing' in sliding seat rowing is rather shorter than on a fixed seat.

In the table, results are shown of the experiments relating to work done in a gig pair, both on sliding and fixed seats. The author regrets that the stroke rate was not taken in more cases, it having been measured in only seven of the experiments. It is, however, not far from the truth to assume that the estimated rates in other cases are not in error more than one stroke a minute.[1]

The trials involved in the table have all been short, ranging from one to ten minutes, and speaking generally a similar rate of working, with a gradual fall in power, could have been continued for some twenty minutes, while, at high pressure, exhaustion would ensue under similar conditions in less than ten minutes.

ROWING IN GIG-PAIR

Average time between easies—3 min.

Average stroke-rate—22.

1	2	3	4	5	6	7
Oarsman	Stroke length in feet.	Ft.-lbs. per str.	Pull.		Mean.	Horse-power.
			Max.	Mean.	Max.	
I. Sliding Seat.						
A	4·9	381	99	77	·78	0·254
B	4·9	418	115	85	·74	266
C	5·2	571	128	110	·86	380
D	4·9	267	90	54	·60	178
E	5·1	399	104	77	·74	272
F	4·6	340	111	73	·66	196
[K	4·2	498	166	119	·72	331]
Mean	4·9	396	108	79	·73	·259
II. Fixed Seat.						
G	4·5	370	127	83	·65	·247
H	4·0	336	128	85	·65	224
I	4·2	343	123	82	·67	227
F	4·5	312	125	70	·56	208
Mean	4·3	340	126	80	·63	·226

The table indicates that Man Power in rowing is about $\frac{1}{4}$ H.P. and varies with individuals and effort between ·2 and ·4 H.P.

[1] With reference to stroke rate, it is worthy of note that the usual custom of counting strokes, in which the number of strokes in the water is taken, makes an error of more than half a stroke, by omitting one swing forward. Thus if strokes are counted for $\frac{1}{4}$ min. the deduced rate is too high by 2 per minute.

The author regrets that up to the present the only experiment in a racing eight was on an Oxford 'Torpid' in its early days of practice. He suspects that during a race of eight to ten minutes' duration the power would vary between ·3 and ·7 H.P.

The figure given in the sixth column of the table is of some importance, since it roughly indicates the form of the stroke. This coefficient, found by dividing the mean pull during the stroke by the greatest pull, gives a measure of the uniformity of the pull. If the usual coaching maxim of "getting the full work on at once and carrying it out to the finish," were literally carried out, the stroke diagram would become a rectangle, and the coefficient unity.

It is, however, probable that the best results would not be obtained with a high coefficient, since the extra work done does not compensate for the additional fatigue, physical and nervous, involved in setting the muscles suddenly at their fullest tension. In addition to this physiological point there is the mechanical one that the efficiency of propulsion is greatest—other things being equal—when the oar is at right angles to the boat, since it is here that the smallest proportion of the work is devoted to generating kinetic energy in the water.

This question of efficiency of propulsion by oars has been made the subject of experiment by the author. To determine this it is necessary to know, in addition to the information afforded by the indicator diagram, the point about which the oar is turning at every part of the stroke. Since the rowlock is moving forwards, and the tip of the blade backwards, some point between these is neither moving backwards nor forwards. This point may be called the Turning Point.[1] A moment's consideration will show that this point changes its position during the stroke, since the blade first encounters 'dead' water, so that the blade tip at first moves slowly, and the turning point is lower down the oar than a moment afterwards when the water has been set in motion.

By attaching a float to different parts of the oar by a string, so that the float was immersed during the stroke, it was possible to estimate fairly accurately the mean position of the turning point, for which a position 36 inches above the tip of the blade was found, i.e., a point only 3 inches above the top of the blade. This shows how comparatively slight is the motion given to the water, a point which will be more clearly brought out below.

In August of last year the author made an attempt to arrive at more exact results on this point by taking a rapid series of photographs of an oar in motion on the same plate, from a point vertically above the boat. Unfortunately, the only time available, about 3.30 P.M. on a cloudy afternoon, was not an ideal one for exposures lasting

[1] It is clearly the projection of the 'Instantaneous Centre Locus' on the oar.

$\frac{1}{360}$ of a second. However, a photograph was taken from which it has been possible to determine some twenty positions, plotting out a whole stroke, with parts of the forward swing on each side (Fig. 6).

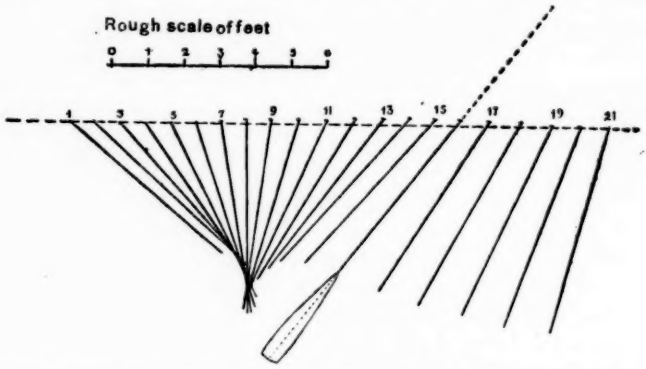


Fig. 6. Motion of an Oar during a Stroke. (Boat is moving towards the right hand.)

The camera was fixed about 23 feet above the River Cam when there was practically no stream to vitiate the results. The exposures were made by a revolving shutter, having three slits, each with an angular width of 5° rotating before a slit of about double that width. The rotation was maintained by such a weight that the speed was fairly uniform during 120 exposures at about 14 exposures per second. As only 26 of the 120 were used, no serious error is introduced by considering the successive photographs taken at equal intervals of time.

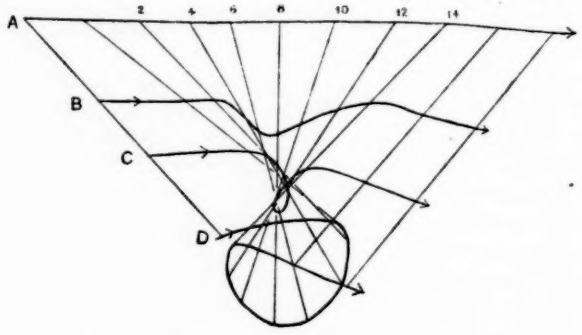


Fig. 7. Curves described by points on the Oars. (*A*), Button; (*C*), 36″ above tip of blade; (*D*), Tip of blade.

Points were marked on the oar at the button, at 36″ above the tip of the blade, and at the tip. The motion of these points is represented by the lines *A*, *C* and *D* in Fig. 7. A projecting part of

the bridge from which the photograph was taken necessitated the camera being tilted slightly out of the vertical, so that there is a corresponding change in the scale of the picture in different parts, but as the important part is the loop of the line *C*, this error is comparatively unimportant.

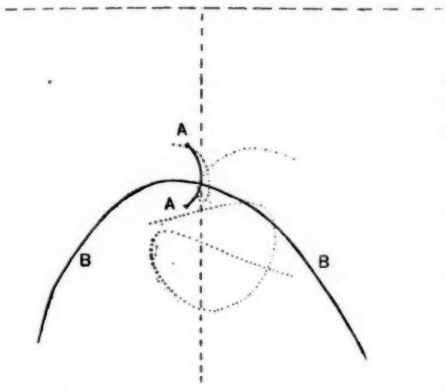


Fig. 8. Motion in space of Instantaneous Centre (*B*) and 'Turning Point' (*A*).

In Fig. 8, *A* represents the locus in space of the turning point determined as the 'envelope' of the various oar positions shown on the photograph, showing that this point moves outwards from the boat in a convex curve facing the bow of the boat.

The curve *B* represents the Instantaneous Centre Locus or point in space about which the oar turns bodily.

Fig. 9 represents the motion of the Turning Point along the oar. From this it will be seen that, starting from a point some 37 inches above the tip, it moves upwards during the first part of the stroke as suggested above, but before the middle of the stroke a curious reaction sets in, and during the rest of the stroke the turning point steadily approaches the tip of the blade, indicating that the blade is coming more and more to rest.[1]

This seems to show that at the beginning of the stroke the blade, which is increasing its distance (see curve *D*, Fig. 7) from the boat's side owing to diminished obliquity, sets up a swirl which moves backwards in the path of the blade, but forwards between that and the boat.

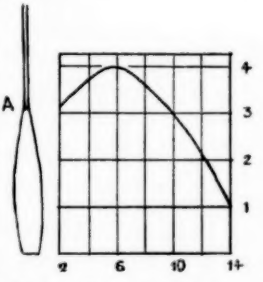


Fig. 9. Position of 'Turning Point' on Oar during the Stroke.

During the second half of the stroke the blade enters this forward moving water and has its motion retarded, taking up thereby some of the energy previously imparted to the water. This point requires further experiment.

[1] The mean position is found to be 36 inches above the tip, a result coinciding with the rough determination with the float eighteen months previously.

In order to determine the efficiency of propulsion it is only necessary to find what proportion of the whole work done is developed as kinetic energy in the water. If the whole pressure of the blade against the water be P, while the centre of pressure moves through a distance a perpendicular to the blade, the kinetic energy delivered to the water is Pa. For every small angle θ through which the blade turns while the turning point is at a distance x from the centre of pressure, the above product $= Px\theta$.

Without entering into details,[1] the angles between successive positions of the oar, given in Fig. 6, were measured and corrected by interpolation from a curve, while corresponding values of x and P were found from Fig. 9 and C of Fig. 4 respectively.[2]

In this way it was found that almost exactly $\frac{1}{3}$ (33·4 per cent.) of the work was left behind in the water as kinetic energy set up by the oar, giving an efficiency of 66·6 per cent. This efficiency coefficient is concerned, of course, only with the rowing mechanism, and takes no account of physiological waste of energy.

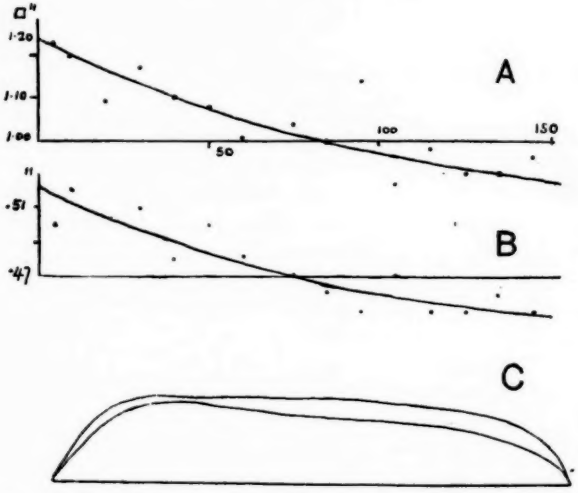


Fig. 10. Fatigue Effect. *A*, Work ; *B*, Greatest Pull ; *C*, Change in Form.

The mechanical efficiency is increased by increasing the size of the blade, so that it can react on a larger body of water, and by increasing the length of the oar, both inboard and outboard, in order to diminish the obliquity at the ends of the stroke. This theoretical possibility is hampered by practical considerations until boatbuilders

[1] See Appendix II.

[2] *C* unfortunately was not the oarsman who was rowing in the oar experiment, but, for a general result, this is of no very great moment.

condescend to turn their attention to making an improvement on present wooden oars and solid outriggers.

In experiments so far described, the only advances made by the new Indicator over the first one have been the greater facility in reducing the results and the possibility of obtaining a mean result from several strokes. Experiments will now be considered for which the continuous record was necessary. These have reference to the effect of fatigue on rowing. Fig. 10 indicates in various ways the growth of fatigue during a continuous piece of rowing. In *A*, ordinates represent the work done during a stroke, whose number from the start is represented by the abscissa. As is to be expected, the strokes vary irregularly, but the steady decrease during the 150 strokes represented is quite clear. *B* represents in the same way the falling off in the maximum pull, and gives a curve almost identical with *A*. *C* shows the change in stroke form during the interval of some 130 strokes, or six minutes' rowing.

It will be noticed that the falling off is most marked during the latter half of the stroke, when the legs and arms take a large share in the work. This suggests that while the powerful system of muscles in the shoulders and back does not easily tire, the legs and arms are comparatively weak.

Fig. 11 represents the change in stroke-form in a four during an interval of 80 strokes. The fatigue-curves in this case are steeper than in the last case, but less regular.

Fig. 11. Fatigue in Four—80 Strokes.

Fig. 12 relates to a journey in a Torpid Eight. This was broken up into about 4 pieces by easies, but again the gradual diminution of power is clear. *C* is an analysis on a larger scale of strokes 210 to 310 during this journey showing the effect of an 'easy.' The diagram shows the maximum pull, and between the two black lines there is clearly a break in which the oarsman has partly recovered his vigour.

With regard to the magnitude of the fatigue effect, in the first case the fall was 18 per cent. in 6 minutes' continuous rowing, 150 strokes; second, 13 per cent. in 100 strokes; third, 22 per cent. in 350 strokes (intermittent). These and other results show that, even in cases where no extreme exertion is called for, fatigue manifests itself, not only as a sensation, but also in diminished

output, and it can easily be imagined how much larger must be the fall in a hard race.

A rough experiment was made to test the use of the indicator for determining the relation between horse power and the resulting velocity of the boat. A pair was rowed between two points, both up and down stream, in order to eliminate the stream velocity. The time and number of strokes was carefully noted. This was

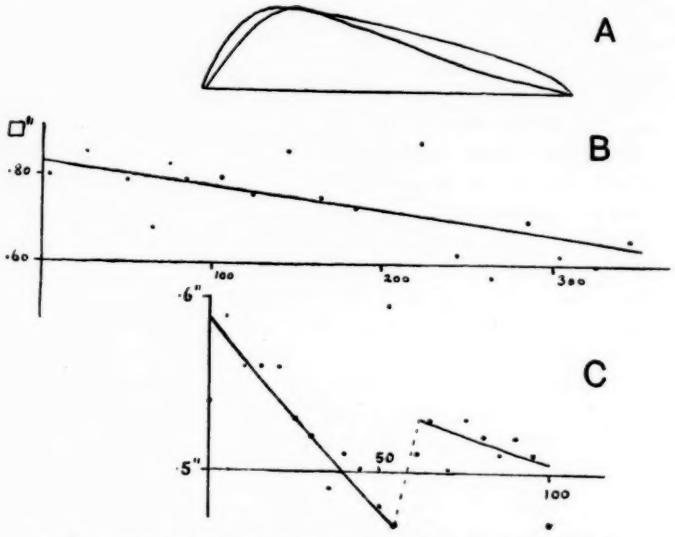


Fig. 12. Fatigue in Torpid Eight. *A* and *B* 350 Strokes, *C* 100 Strokes.

carried out once with very little exertion and again working hard. The measurement of the diagrams, combined with the other observations gave the H.P. corresponding to a certain relative velocity. Assuming H.P. $\propto$ (Vel.)^x, x was found by this experiment to be 2·5—a result probably too low.[1] For the experiment to have a scientific value it would be necessary to indicate both rowers:

The experiment is alluded to as suggesting the possibility of an investigation that might lead to useful results, and although the author has given a description of some experiments which he hopes may be of some interest to the rowing man and also to the physiologist, yet he feels that at every point, more extended, and often more careful experiments, for which he has now no longer the opportunity, would be of interest and importance to science.

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TEMPLE OBSERVATORY,
RUGBY.

[1] See Appendix III.

APPENDIX I

The Oar Used and Constants of Indicator

The axle of the Indicator is 1″ below the button. Midway between the hands is 41·5″ above this point, and the tip of the blade is 102″ below it. The centre of pressure of the blade was calculated on the rough assumption that the pressure at a point varied as the velocity, and it was further assumed that the turning point occupied its mean position 36″ above the tip.

The centre of pressure thus calculated is 9·6″ above the tip.

Hence (see Fig. 13) $\frac{P}{83}=\frac{F}{184}=\frac{R}{267}$.

The instrument was calibrated against a spring balance, using a lever with a mechanical advantage of 3·07, and a series of experiments with the spring used throughout, showed 1″ on diagram = 319 lbs. pressure or a pull of 220 lbs.

The radius of the drum is 2″·02, so that 1″ of base line on the diagram represents motion of $F=1\cdot71$ feet.

Finally, 1 square inch of diagram represents $319\times\frac{184}{267}\times1\cdot71$ or 377 *ft. lbs.*

APPENDIX II

Efficiency of a Stroke in Rowing

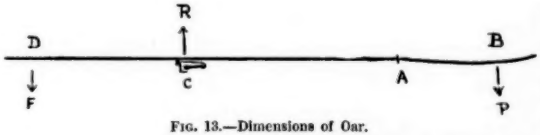

Fig. 13.—Dimensions of Oar.

Let A be 'turning point' while oar turns through an angle $d\theta$, B centre of pressure of the blade, C the button, D centre of pull on handle of the oar.

Work delivered to water $=P\times AB d\theta=R\frac{CD}{BD}\cdot AB\ d\theta$.

Work done by man $=F\cdot CD\ d\theta=R\frac{CB}{BD}\cdot CD\ d\theta$.

So assuming B a fixed point,

$$\text{Efficiency}=1-\frac{\int R\cdot AB\ d\theta}{\int R\cdot CB\ d\theta}=1-\frac{\Sigma R\cdot AB\cdot\Delta\theta}{\Sigma R\cdot CB\cdot\Delta\theta}\ \text{practically.}$$

$\Delta\theta$ between successive positions of the oar was measured for all pairs of positions, and a curve drawn, from which corrected values were deduced.

R was measured in an arbitrary scale for corresponding values of θ on C, Fig. 7, and AB on Fig. .

A table was then constructed as under:—

No.	θ.	$\Delta\theta$.	AB.	R.	$R\cdot AB\cdot\Delta\theta$.	$R\Delta\theta$.
×	×	×	×	×	×	×
7	42·8	8·76	36·6	14·3	4580	125
×	×	×	×	×	×	×

Then by summation:—

$\Sigma R\Delta\theta=1049\cdot7$, $BC=92$

$\Sigma R\cdot AB\cdot\Delta\theta=32570$

$$\therefore\ E=1-\frac{32570}{1049\cdot7\times92}=\cdot666.$$

APPENDIX III

Resistance and Horse Power as a Function of Velocity

In the experiment referred to in the article, the H.P.'s in the two cases were ·294 and ·151 respectively, while the velocities were in ratio 421 to 321.

If H.P. $=k(Vel)^x$ $x=\dfrac{log\ 294-log\ 151}{log\ 421-log\ 321}=2{\cdot}5$.

Comparing this with examples of towing vessels this would appear rather low.

There is, however, a difference between towing, and propelling with internal mechanism.

In former case if Resistance $=kV^x$ where V = velocity,

Horse-power $=kVx\times V=kVx+1$.

In the latter case, in which the vessel is propelled, for example, with a screw or oar, the force of propulsion is obtained by generating momentum in the water. Making the apparently legitimate assumption that the mass of water encountered by the propeller varies as the velocity of the vessel (probably more correct in the case of the screw than with an oar), since the rate at which the propeller is passing the main body of water is V.

If Resistance $=kV^x$, again

H.P. $=lVx+1+mV^{2x-1}$ where l and m are constants.

So that unless $x=2$, the H.P. required no longer varies as $Vx+1$.

I have nowhere seen this pointed out, and should be glad of any information on the subject.

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II

Scientific Proofs *versus* 'À Priori' Assumptions

UNTIL the great impetus had been given by Darwin to the acceptance of the Doctrine of Evolution, by the publication of his "Origin of Species," natural science mainly consisted of the observation of facts. Thus, old text-books of botany contained the names and descriptions of the various organs of plants with little or no attempt to deal with their physiological uses, much less with their origins. The old idea, that 'species' were fixed entities, created as we now see them, with all their organs complete, led men tacitly to assume that such descriptions were all that was necessary. Botany mostly consisted of the accumulation of morphological facts to aid the systematist. If any suggestions were proposed as to the purpose of this or that organ, the use of which was not very obvious, 'teleological guesses,' as they might be called, were thought to be amply sufficient to account for them. The Bridgewater treatises may be taken as the type of that old method of interpretation of 'uses,' which was, in fact, simply that of *à priori* assumptions without any strictly scientific bases to go upon; by which I mean neither any accurate observations nor experiments, wherewith to verify the supposed uses.

Like that of teleology, it has now come to be generally recognised that the inherent fallacy underlying metaphysics is due to the want of external observations and experimental proofs; so that no worker in natural science can well be a metaphysician at the same time, for the methods of proof—if any such term can be applicable to metaphysics at all—lie in opposite directions. The scientific student should be satisfied only with objective facts; the metaphysician is contented with subjective imaginations.

Darwin published "The Origin of Species" in 1859. This work at once broke down the old ideas of the fixity of species, as having been created such, and having unalterable forms; but the question immediately arose: How are new forms worked out by evolution in nature?

Darwin and Dr Wallace simultaneously propounded the theory of Natural Selection. Though differing in some important points, both based their conclusions on the following statements:—

(1) That more offspring are born than can ever live to maturity and so leave fresh offspring.

(2) That no two individuals of the same kind are ever abso-

lutely alike, in consequence of their 'individual differences'; and these supply material for natural selection to act upon.

(3) That when a being migrates into a new environment, this somehow induces variations to arise in the offspring, which then, it is supposed, vary 'indefinitely,' *i.e.*, in all sorts of directions; but only those best suited to the new surroundings live, all the rest die. Professor Huxley described this process of natural selection of the fittest to survive, as a system of 'trial and error.'

(4) That the rule is that plants of which there is a numerous population are best suited for giving rise to new varieties when some geological catastrophe alters the conditions of their existence, *i.e.*, without migration.

(5) So that those individual plants which possess new variations of structure which render them the best fitted to survive, will do so under those new conditions of existence.

Now the statement No. 1 can be abundantly and easily proved to be true. No. 2 is also quite true. No. 3 is not true so far as varying 'indefinitely' is concerned. This was an *à priori* assumption, which has never been verified, no facts having ever been brought forward to sustain it. No. 4 is also unsupported by any facts; on the contrary, gregarious plants as a rule supply no varieties. No. 5 is a reasonable deduction or *à priori* assumption, had there been any facts to start with. This not being the case the assumption falls to the ground.

On the other hand, observations and experiments prove that all variations which arise in plants are the result directly or indirectly of responses or adaptations to external influences. Such are always 'definite,' to use Darwin's expression, in every case; and whenever this is so, as he himself admits, "a new variety would arise without the aid of Natural Selection." Instead of this being the exception and indefinite variations the rule, as he supposed, the truth is, that definite variation is a natural law admitting of no exceptions at all. Indefinite variations in nature were a pure assumption.

Where, then, is there any opportunity for natural selection to act? It is the universal process in the struggle for life in nature. A neglected lawn, now existing, affords the writer an excellent object lesson. Daisies hold their own over large areas where about four years ago there were none; but among them are sharply defined places in which *Poa annua* or other grasses utterly refuse admission to anything else. Another district is invaded by *Alchemilla arvensis;* yet another consists of large plants of dwarf Dutch clover, while *Achillea millefolium*, with its insidiously creeping stem, constitutes a compact carpet; so does *Galium verum* in many places.[1]

[1] In a lawn made on the side of a heath *Galium saxatile* formed the 'turf' to an almost entire exclusion of everything else.

On the other hand, where grass is allowed to grow to 2 or 3 feet high, all such dwarf plants rapidly disappear. The former conquerors over mown grass cease to be victorious and disappear where their own victims previously had a hard time of it.

Similarly in a neglected kitchen garden a row of peas, unstaked, have succumbed to chicken-weed, *Urtica urens*, *Solanum nigrum*, groundsel, &c.; but a row supplied with sticks have grown up in defiance of their enemies.

Thus natural selection soon decides what shall be smothered and which shall proclaim itself the fittest to survive. It thus brings about the distribution of plants, but no one has ever shown that its sphere of action in any way concerns the origination of specific characters.

The three principal *à priori* assumptions upon which Darwin's theory was based, then, are as follows:—

(1) The first assumption was that individual variations are the source of varietal variations ("Origin of Species," 6th Ed., p. 34).

(2) The assumption that plants vary indefinitely under changed conditions of life.

(3) The assumption that natural selection eliminates the unfit and retains the fittest to survive among indefinite variations, *i.e.*, of numerous offspring of any one and the same species.

Darwin based his theory mainly upon these assumptions, and more than one of his followers seem to rest satisfied when they say that there is no evidence in the present state of our knowledge, and think that is an all-sufficient answer to the demand for scientific proof. This self-satisfied attitude of professing ignorance of any facts, wherewith to support the theory, seems to me to be the most remarkable, and I would add, deplorable position possible, for any one professing to be a scientific man, to take. I can only regard it as an alarming evidence of the evil growth of a belief in *à priori* assumptions, as if they were scientific methods of proof.

What is the cause of this eager pursuit of subjective will-o'-the-wisps, instead of the study of objective facts? Is it that it is so much easier to sit still and imagine how things may go on in nature than patiently to investigate and accumulate storehouses of facts as Darwin did? Unfortunately he attributed to natural selection a rôle to play which it could not undertake. The result was inevitable. The facts of variation under changed conditions of life are so obvious that his books teem with instances, and then natural selection drops out of sight. When, however, he is arguing on behalf of natural selection, then definite variations seem to be allowed to drop into the background. Hence his "Origin of Species" and "Animals and Plants under Domestication" are full of these two opposite drifts.

We must not forget that the main result of Darwin's writings was to substantiate not natural selection, but the doctrine of evolution. It is this for which our thanks to him cannot be too great; natural selection was but an hypothesis wherewith to account for it, though as we have seen it was based not on facts, but on assumptions, so far as it was supposed to be connected with the origin of species.

Darwin's methods have, unfortunately, led others to follow them. Thus the late Mr Romanes wrote a long and elaborate paper on "Physiological Selection."[1] It was really based on an *à priori* assumption; for he proposed testing his theory or getting others to do it after he wrote and published it. As might be foreseen no subsequent proofs were forthcoming. It still remains what it was at first.

I would venture to beg of writers when advancing a theory, to try and avoid ever using the words 'may be,' 'might be,' 'would be,' or 'must be,' phrases so freely used by Dr Wallace and Dr Weismann. Let them stick to what 'is,' collect innumerable facts in support of any contention, and they will then find little or no thinking or reasoning out will be required at all. For facts soon tell their own tale and quickly make havoc of *à priori*, gratuitous and baseless assumptions.

Indeed, it seems to me that we are rapidly falling back into a position like to, if not the same, as that of the teleologists of the last century. Thus it is frequently asserted by Darwinists that holly has prickly leaves up to a certain height, in order to prevent cattle browsing upon them, and that when the trees grow out of reach, the leaves are non-spinescent. What is this but pure teleology? Only, instead of saying the Creator made them so, as our forefathers would have done, they attribute this supposed result to natural selection. Instead of studying the natural condition of life in which spiny plants grow, they make the above easy but unwarrantable *à priori* assumption, forgetting, or not taking the trouble to observe, whether the holly actually does keep off cattle,—which it does not, for cows are particularly fond of eating holly boughs, as I know to my cost, some well-trimmed bushes having been spoilt by them. Moreover, in early summer when new holly leaves appear, just when they might tempt a cow, they are entirely unprotected, for you may put a bunch of leaves in your mouth and you would not know that they were going to be spiny at all. Lastly, hollies often grow to twelve or more feet high, and retain spiny leaves throughout. So, too, is the same teleological argument held with regard to nettles and their stings, but (alas for the Theory of Protection) numerous species of caterpillars live on

[1] *Journ. Linn. Soc. Zool.*, vol. xix.; 1886.

stinging nettles. Cows often eat them, and man makes 'soup' and 'spinach' of them.

Natural selection, being a purely imaginary agent, is as easy to manipulate as is the Creator's name to account for phenomena, where no proof can be given. As soon as one asks for some grounds for such inferences, the retort comes, "In the present state of our knowledge, it is admitted that there are none!"

It may be now desirable to state what scientific proofs consist of.

There are two lines of evidence possible in support of some deduction arrived at for the interpretation of some natural phenomenon.

The first and best is experimental verification. If you find the result comes as you expected when you have supplied the conditions which, according to your deduction, you supposed to be capable of producing it, then that is all-sufficient and proves your theory to be fact.

Take the case of spinous plants. One first observes as a matter of fact that spiny processes are particularly common in plants growing in arid soils and a dry atmosphere, whereas they do not appear among marsh or aquatic plants. It is always coincidences that one first looks for. Then the question arises, Is the spiny structure in any way due to these external conditions of the environment? Now the test is to grow normally spiny plants in a good soil with plenty of moisture and in a moist atmosphere. Then follows the anticipated result that spines are no longer produced. If they be branch-spines, then the branches grow out into leafy shoots. If they be reduced and spinescent leaves, as in barberry, they at once develop into true leaves. To be quite sure you test it with other plants, and the same result follows. Your theory, therefore, is a proved fact, which henceforth is recognisable as an established natural law.

A different line of proof is required when a deduction cannot be verified by experiment. It must then be established by induction, or the accumulation of probabilities in its favour, until the converse is practically unthinkable. This is the chief line of evidence for establishing evolution as set against the old form of natural theology and teleology.

That I may not lay myself open to the charge of propounding what I have not done myself, I will take my deduction from an observation made in 1870, that irregular flowers are the result of the mechanical action of insects visiting them for honey or pollen. This conception cannot be proved experimentally, as it is impossible to make a regular flower become an irregular one. Another deduction has been drawn by others, namely, that gravity has been the cause of the enlarged lower petal or lip. The

question is, which can bring forward the greater number of correlated facts in support of these two deductions, respectively, *i.e.*, so that they may be supported by inductive evidence? Now gravity is claimed as acting on one petal, but it cannot account for the erect stamens and style in the dead-nettle, and all the rest of the features of the flower. My suggestion is that the irregularity seen in all the organs of the flower are brought about by one and the same cause,—the mechanical action of the insects which visit it.

Assuming the deduction as a working hypothesis, I bring forward (1) abundant evidence to show that protoplasm responds to mechanical forces and builds up structures to meet the strains to which it is subjected; (2) the ribs of the calyx and its form corresponds precisely with the distribution of forces, as seen in Salvias. The form of the corolla is just what would result if it be supposed to be plastic and moulded to the form of an insect, as in *Anhatoda*. The stamens are erect or declinate, in correspondence with a flower having a landing-place on the corolla or not. The honey-gland is situated, and the 'guides' directing to it, precisely in adaptation to the insect visitor. In fact, the entire flower is simply a vast accumulation of innumerable coincidences, all conspiring to one and the same end. It is this which constitutes inductive evidence: while all the correlations are based on the well-known properties of protoplasm, the accumulation of coincidences affords a probability of so high an order as to amount to a 'moral conviction,' and such—all logicians admit—is equivalent to a 'demonstration.'

Similarly, I maintain by inductive evidence that Monocotyledons have descended from aquatic Dicotyledons.

These two lines of proof are amply sufficient, as scientific evidences, to establish the truth of any theory, and convert it into a natural law.

I think I have now said enough to show the utter incompetency of *à priori* assumptions to prove anything at all, of themselves. They are simply deductions, without verifications, and as such remain utterly valueless, and instead of advancing any branch of science, do but retard it, as long as they are accepted without verification. Their danger, however, is subjective not objective. A deduction or *à priori* assumption is useful only as the first step. It must be verified. It is the fatal facility of guessing inherent in mere thinking, irrespective of facts—to collect which is a laborious process, in which Darwin set so grand an example, and upon which evolution was based. This is the imperishable result of his labour, while the theory of Natural Selection, as having anything to do with the Origin of Species, was a quite subordinate matter, and has turned out to be a broken reed to rely upon.

GEORGE HENSLOW.

DRAYTON HOUSE, EALING, W.

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III

Natural Gas in Sussex [1]

IN introducing my subject, I do not think it necessary to make any elaborate references to instances of discoveries of Natural Gas in England and abroad. Suffice it to say, that manifestations of natural inflammable gas have occurred in almost every country and geological formation throughout the world, and have frequently been put to practical use.

I will, however, mention what seems to have been one instance of its appearance in London, quaintly recorded by one, Mathew Paris, about the year 1256. Under the head of 'A Sudden Subterranean Explosion,' the chronicler says, "About this time, as some workmen were digging out the bed of an aqueduct in London, to clear the bed of mud (for the water had ceased to flow) a sudden explosion burst forth from the ground accompanied by a flame similar to the fire of hell, which, in the twinkling of an eye suffocated several of the workmen, killing one of them on the spot, and so burning, maiming and disfiguring others that they were entirely useless to themselves ever afterwards. There were some who said that this explosion occurred as by a miracle, because those men were engaged in servile work at an improper hour in the evening." (It quite sounds as if the Factory Acts had been anticipated in these days.)

This interesting record has a somewhat similar parallel in the County of Sussex, and I may quote it for the benefit of well-sinkers personally, and master well-sinkers who may come within the provisions of the Employers' Liability Acts.

I am indebted for the account to Mr Henry Nicholls of Deal, an owner of property at Hawkhurst in West Sussex. He states that between the years 1836 and 1840 a well was sunk at Hawkhurst to a depth of 98 feet. After passing through a certain amount of heavy sand, a blue clay of a very oily flaky nature was met with, mixed with yellow and red streaky clay. This continued to the bottom of the 98 feet. An artesian boring was then commenced, the workmen working by candle-light. Having bored some 50 feet more, or 148 feet from the surface, the augur struck a rock and fell

[1] A paper read at the Conference of the South-Eastern Union of Scientific Societies, Town Hall, Croydon, June 3, 1898.

into a cavity. An inflammable gas immediately ascended, which got ignited by the lights of the workmen. Two men were immediately killed, and as an eye-witness says, the gas burned slowly up the well till it came near the top, when coming in contact with the outer air, it burst out into a sheet of flame, some 20 feet high. It then slowly burned itself out. The water in the well was useless, and Mr Nicholls had the well filled up.

This seems to have been an instance of an inflammable gas occurring in association with strata containing a rock-oil, the gas itself accumulating in a cavity, or what is called by the Americans a 'pocket.' It serves to show that it is unwise for well-sinkers to use artificial lights at the bottom of a well when boring for water, except perhaps in properly constructed mining-lamps.

Another somewhat interesting occurrence took place near Ticehurst Road, Sussex, about six or seven years ago. There is a certain low-lying field, called the 'Bogs Brook,' close to the Ticehurst Road Station of the South-Eastern Railway. It is a marshy spot, and sometimes large bubbles of inflammable gas continuously rise and break on the surface of the pools. One Sunday in a particularly dry summer when the bog was dried up, some boys were about to enjoy a clandestine smoke of tobacco, when a match thrown down suddenly ignited something believed to be inflammable gas. The boys ran away, and the whole field was soon a mass of flame; the peat of the bog also took fire. I am told that the spot, which is in view of the railway, was visited by thousands of people at the time; it burned for a week or more, when some heavy rains soaked the land and put out the fire.

These subjects, although interesting, have but slight interest as compared with the more important occurrences of inflammable gas coming from artesian borings, with a continuous flow during months and years, and existing under a high degree of pressure. Inflammable gas is mentioned by Mr Henry Willett, F.G.S., in his account of the famous Sub-Wealden boring at Netherfield in 1875, as occurring in the Purbeck Strata, and at a short distance above certain strata in the upper Kimmeridge Clays, recorded to be very rich in petroleum. This seems to be the first record we have of a class of gas which has now again been met with in East Sussex. Of course I do not now speak of gases emanating from petroleum at high temperatures, but of certain gases usually found in a free state in association with petroleum, and perhaps, therefore, owing their origin to some common causes and conditions.

An inflammable gas was met with in a boring made by Messrs Le Grand & Sutcliffe (the celebrated hydraulic engineers) at the Heathfield Hotel, Waldron, Sussex, in rocks at a horizon very little

higher than that at which it was discovered by Mr Willett. The foreman of the works made some experiments in piping off the gas. No water was discovered, so the boring was closed up and no more was thought of it until the same firm of engineers, by order of the London and Brighton Railway Company, again made another boring, about 100 yards to the south, commencing in the railway cutting about 43 feet in depth below the level of the top of the former boring. In this boring, but at a greater relative depth, gas was first noticed. I say first 'noticed,' because it is now certain that gas first began to come into the boring at a higher level, perhaps at the same relative level as in the former boring. The rush of gas became greater as the depth increased, and when tested at the top of the bore-tube with a light by Mr E. Head, the station-master at Heathfield, a column of flame sprang up to the height of about 16 feet, and was with great difficulty extinguished. A certain amount of water was discovered, but not sufficient for the Railway Company's purpose, and the boring was abandoned, nearly all the lining tubes being withdrawn. Notwithstanding the partial blocks due to the falling-in of the sides of the bore-hole and the pressure of a great column of accumulated water in the bore-hole, the gas still continues to flow from the bore-tube in considerable quantity. It has been calculated that the pressure of the gas at its source at the bottom of the tube cannot be less than 135 lbs. to the square inch.

It is perhaps somewhat providential that some obstruction has happened to prevent the enormous loss of gas that would have taken place had the tube been left entirely open during a period of, now nearly two years. The Railway Company have screwed a cap on to the end of the tube, with a small half-inch outlet, from which the gas has been allowed to flow continuously.

With the kind permission of the Railway Company, whose officials are giving every kind assistance and facility, my friend Mr Lewis, C.E., F.S.A., and myself have conducted various interesting experiments with the gas; and permission was obtained from the Company for a demonstration of the gas when used in various burners on the occasion of the visit to Heathfield of the Brighton and Sussex Natural History and Philosophical Society on June 11.

Respecting the origin of the gas, we look in vain to the rock-details of the boring for information. It is true that certain small beds of lignite occurred in the section, but one cannot account for the enormous supply and pressure of gas on any theory that the gas emanates from these beds. A portion of the lignite in one of the beds occurred at the depth of 347 feet (at the junction between the Fairlight Clays and the Purbeck Beds), consisting of blue sandy marl-rock with bands of lignite, and has been analysed by Dr J. T. Hewitt (Professor of Chemistry at the Technical College, East

London) on behalf of the Railway Company. He reported to the Company that the lignite contained :—

Moisture	4·90
Volatile matter	15·55
Fixed carbon	1·74
Ash	77·81
	100·00

This record I take it is about the usual result of the analysis of lignite, and I fear throws very little light on the subject.

The greater probability seems to be that the gas is derived from either the Purbeck Beds or the Kimmeridge Clays by percolation through the comparatively porous strata above. The Purbeck beds are known to contain a certain amount of petroleum and bituminous matter, one bed being particularly rich ; but far richer deposits are in the deeper lying Kimmeridge Clays, immediately above which Mr Henry Willett discovered the gas.

This matter, however, as also the subsidiary one of the association of petroleum, is one which can only be determined satisfactorily by means of a deeper boring. The Heathfield borings are much shallower than the bulk of those in North American gas wells, which not uncommonly exceed 1000 feet. The gas which occurs in so many of these American wells is usually the forerunner of a spring of petroleum, and it is possible that the occurrence of gas in large quantities at Heathfield may indicate that there is a larger supply of oil in the petroleum-bearing strata beneath than has been before known to occur at the same horizon in other places.

Dr Hewitt has also reported to the Railway Company on the gas, which, he states, is composed of three constituents :—

Marsh Gas	91·90
Hydrogen	7·20
Nitrogen	·90
	100·0

The first two of the above gases, it may be remarked, are inflammable, but burn only with a blue non-luminous or comparatively non-luminous flame. Nitrogen is not an inflammable gas. It is clear, therefore, that there is nothing in the analysis which can account for the illuminating power of the natural gas at Heathfield, for it burns with a brilliant yellow flame. The gas when burnt in an ordinary 'batswing' or 'flat flame' burner is so luminous that the casual observer would not remark the difference between it and ordinary household gas (although the difference does actually exist). Therefore we must suppose either that some luminous property in the gas did not present itself in the sample taken away by Dr Hewitt, or else that some variation has occurred in the constituents

of the gas (a feature which appears to be not uncommon in the Natural Gas of the United States, though I believe not quite to the same extent). I myself and others have seen the gas burning at many times and at different periods, but this non-luminous phase has not presented itself to me or to anyone with whom I have yet met.

I will therefore confine my remarks to the gas in what, out of respect for Dr Hewitt, I will call its normal or luminous phase.

Under these conditions the gas has been carefully analysed by Mr S. A. Woodhead, B.Sc. (Public Analyst for Sussex and Professor of Chemistry at the Agricultural College, Uckfield), and I here beg to record my thanks to him for the time and trouble he has taken to secure the accuracy of his determinations. He constructed his laboratory on the spot at Heathfield, an unlimited supply of gas being supplied to him direct from the bore-hole by means of tubes, and he has taken care to check his results.

The analysis, which is shortly to be published in the *Quarterly Journal* of the Geological Society, speaking roughly, agrees fairly well with that of Dr Hewitt, so far as the presence of Methane (or Marsh gas) is concerned, but Mr Woodhead's analysis reveals the presence of certain hydro-carbons which may make all the difference in accounting for the undoubted illuminating power of the gas. Other important differences between the analyses are outside of the scope of the present paper.

I may remark in general that the Natural Gas, in common with the American Natural Gas, so frequently discovered in association with petroleum springs, is chiefly remarkable for its great heating power when mixed with a large proportion of air. Its main usefulness may thus be said to lie in the direction of lighting by incandescent burners, fuel in manufacturing-engines, and general household purposes.

Tested by Messrs Thorpe & Tasker's photometer, the illuminating power approximates 9½ standard candles. Mr C. E. Masterman, secretary of the Denayrouze Light Syndicate, has kindly tested the gas (forwarded to him in an india-rubber bag). He says (report 3rd, June 1898), he obtained very fine results, and that with a special burner on the Bandsept principle and a C.X. Welsbach mantle, he has obtained 72 candles with a consumption of 2½ cubic feet of gas, at a pressure of 2½ inches, which thus works out to 29·6 candles per cubic foot. This is 15 to 20 per cent. better than London coal-gas.

In using Bunsen burners for heating purposes, it is found best to use about 8 to 10 parts of air to one of Natural Gas. These gases only reach their maximum heating power when a due amount of air is mixed with them, to insure complete combustion.

The Waldron gases are exceptionally fortunate in containing no

impurities of any kind. The chief and main object of the development of a Natural Gas field, is to be found in the use of the gas for fuel in the generation of motive power for various manufactories. In North America there are at least sixteen States using Natural Gas. In the State of Indiana, U.S.A., alone there exist about three hundred Natural Gas companies. The question of the maintenance of the supply is an all-important one. In the American States laws have been passed with a view to economise the use of the gas, which, as now at Heathfield Station, is allowed to rush or burn to waste. Experience abroad shows that even the most abundant fields of gas are capable of exhaustion. Thus in Indiana in 1889, the average initial rock-pressure of the entire field was 325 lbs. to the square inch; in 1896 it had fallen to 230 lbs. to the square inch. On the other hand, such gas has been used for ages in China for the boiling down of brine for making salt. It seems a pity that the two borings at Heathfield should be allowed to seal themselves up without some effort to test the practical utility of this field of Natural Gas. If the tubes were again cleared and relined with perforated tubing, and the water in the tube was pumped out, the important question of the value of the field, which promises so well, might very soon be arrived at without much expense.

Whatever results may accrue from these lighting and heating properties, or whatever the discovery may point to in a commercial direction, the fact nevertheless remains that the discovery is a subject both interesting and instructive, and, I think, worthy of consideration at this conference.

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IV

'Nuclear Reduction' and the Function of Chromatin

EVERY student of biology who remembers the quickening effect of the "Principles of Biology" on his own youthful thought will have greeted Mr Spencer's announcement that a second edition of this great work is in progress, and welcomed the return of the master to his old pursuits and studies. None the less is it a duty to point out that one section of the instalment which we read with so much interest in the May number of *Natural Science* (vol. xii. p. 307) presents a view of the occurrence of 'nuclear reduction' as being an antecedent of fertilisation, which is inconsistent with clearly ascertained facts, and a physiological explanation that is inapplicable to some of the cases where it takes place.

I

The following remarks should have found their correct place in a paper on "The fundamental Principles of Heredity," published in *Natural Science* for October and November last, but were omitted not to overweight it with details of a somewhat abstruse character, and as lying apart from the main object of the study—the tracing out of the cellular pedigree of the organism, with insistence on the point that 'collateral cellular transmission' was operative in all higher organisms. And I would ask the reader to refer to my previous paper in connection with the present one.

'Nuclear reduction' is an easy process to define. When a nucleus is about to divide, its formed matter resolves itself into a definite number of segments; these split each into two, one of which is destined to either of the daughter nuclei resulting from the division. These segments have received the name of 'chromatomeres' or 'chromosomes'; we shall use the shorter, as the more generally received one, though the longer does not carry with it the numerous hypothetical implications, mostly wrong, of the other. Usually, a nucleus on the approach of division reveals the formation of as many segments as entered into it at its formation; and thus the number of segments remains constant from (cell-) generation to generation in the same species; but at a certain point in the life-cycle the number of segments appearing on division is smaller than at the previous divisions of the cells of the parent-cycle; and this is called 'nuclear reduction.'

Three modes of nuclear reduction have been described:—

Case I.—In Elasmobranchs, some Amphibia, and Mammals; in Flowering Plants, some Archegoniates, and Fucaceae. The nuclear network resolves itself at once into half the previous number of segments; certain modifications occur which do not alter the principle of the matter. Here there is clearly no true 'reduction,' any more than if a man received ten shillings from his father and left five florins to each of his two sons.

Case II.—In *Sagitta* and *Ascaris*, in some Gasteropods, and in Liverworts, a modification of this process has been made out, which was first noted by Boveri and Hertwig in 1890; while its relation to Case I. was dwelt on by me in 1891. Here the reduced number of segments appears in a certain cell; but after the first splitting for the coming division a second splitting of each occurs, so that at the first division of the nucleus each daughter nucleus receives the reduced number of segments ready split for the second division; and at the second division the again new daughter-nuclei receive each its own set. This is merely a displacement in time of the two successive splittings for two successive divisions of the nucleus. Similarly, often when a cell is going to undergo two or more successive divisions, the nucleus undergoes the consecutive divisions before the cytoplasm divides once. The reduction is essentially of the same character as in Case I.

Case III.—The nucleus about to divide reveals a number of tetrads or groups each of four segments; the number of groups is only half the previous number of segments, and consequently the total number of segments is twice the original number: the nucleus divides twice, and at each division the several groups are halved between the resulting nuclei; hence it is clear that at the second division each nucleus contains one segment from each group—*i.e.*, a number equal to the number of groups, and half the original number of segments in previous cells of the cycle.

Concerning the details of Case III. there are many conflicting observations, many inconsistent explanations, many vain hypotheses based on the assumption that this is the typical mode of nuclear reduction to which all others are to be forced to conform by some Procrustean process. But we shall not go into these; the explanations that cover Cases I. and II. will cover Case III. also, even if in the last there be something additional left over; but the explanation of this something can well wait until the facts themselves are better made out.

Where does nuclear reduction occur? In Metazoa, usually at the first of the two cell-divisions that give rise to a brood of four spermatozoa, or to the oosphere and the three polar bodies (abortive oospheres) respectively: that is to say, at the inception of the forma-

tion of the sexual pairing cells. The extension of this by zoologists to all cases was tempting; and the demands of the Weismannism of the '80's made this extension appear imperative: reduction or excretion processes in gametogeny were diligently sought for, and of course found everywhere; and in '91 (pp. 62-3) I enumerated and discussed as many as fifteen which had been accumulated in defiance of morphological homology or physiological equivalence. In the same paper, I discussed the question of nuclear reduction from the then state of our knowledge; and pointed out that in Flowering Plants reduction occurs in the pollen-mother-cell, and that this is the equivalent of the asexual spore-mother-cell of Archegoniate Cryptogams (Ferns, Mosses, &c.). "We must remember that the reduction takes place in the pollen-mother-cells of Flowering Plants, which are themselves homologous with the mother-cells that form tetrads of asexual spores in Archegoniate Cryptogams; hence we may be allowed to conjecture that reduction also takes place in the latter group; and by parity that it is not confined to gametogonia[1] [= the mother-cells of a brood of gametes]."

At the time there was only one case, that of a liverwort, that had been at all fully worked out, but since then, we have learned that in the ovule of Flowering Plants reduction takes place at the first division of the primitive nucleus of the embryo-sac; and that in the Archegoniatae without exception, reduction takes place at the inception of the formation of the tetrads of spores, not at that of spermatozoa and oospheres, the equivalents of the sexual cells of Metazoa. Moreover the spore of Mosses gives rise to the leafy plant, capable of indefinite vegetative growth and propagation; that of the Fern to the Fern-scale, which in Gymnogramme, for instance, is perennial also. Thus nuclear reduction is not a process that finds its explanation in the formation of cells specially adapted for 'sexual' (sit venia verbo[2]) fusion or gamogenesis.

I may be permitted to refer to my recent paper ('97) for the exposition of the existence in Metazoa of a long cycle of colonial cell-divisions, alternating with a short one of protistoid brood-

[1] '91, p. 57-8. The sentence closes thus: "but will be found in all mother-cells destined by multiple fission to give birth to a brood of reproductive cells." Of course the latter part of the conjecture has not held good, but the former part has maintained itself: namely that reduction takes place in Cryptogams at *spore*-, not *gamete*-formation. The anticipation thus formulated by me in 1891 was repeated by Overton in 1893, and its enunciation has been ascribed to him by Strasburger ('94a, p. 291; '94b, p. 825), while later the error has been continued by Wilson ('96, p. 196). I did not think such a question of priority worth noting by itself, but take this opportunity of correcting the mistake.

[2] The word 'sexual' has two distinct meanings; the one relating to the fusion of two cells, &c., into one, the other the differentiation of such pairing-cells into two unlike categories such that cells of the one will only pair with cells of the other. 'Sex,' 'sexual differentiation,' 'sexual processes,' are terms as often used in the one sense as in the other; and we may easily avoid the confusion by describing the former as 'pairing processes,' or 'fusion processes,' and the like, and using the additional adjective 'binary' with 'sex,' 'sexual,' to distinguish the latter meanings of the terms.

divisions producing the sexual-cells. In Metaphyta, there are two such alternating cycles of colonial and protistoid growth, the Moss-Plant or Fern-Scale producing the sexual cells, and the Moss-Urn or Fern-Plant producing the asexual spores. In 1891, I wrote of nuclear reduction:—"We may perhaps regard it as an adaptation to prevent the undue multiplication of chromatomeres in the zygote, and the cells produced therefrom." This view has been elaborated by Strasburger; but it will be better, as we shall see, to explain it in another form than his. As, normally, each nucleus exhibits on its division the same number of segments that it had on its formation; the fertilised egg, oosperm, zygote, or whatever we please to call a cell formed by the fusion of two, on its division will present twice the number that were present in either of its two original constituents. If, then, at each sexual fusion this doubling continued, the number of nuclear segments in each cell would increase indefinitely in geometric progression, which is, of course, out of the question: a reduction must take place somewhere. This necessary reduction takes place at the first resumption of protistoid multiplication. In Metaphyta, where there are two such resumptions, this is obvious; in Metozoa there is only one such resumption, which coincides with the formation of the (protistoid) sex-cells,[1] and it is this mere coincidence that gave rise to the idea that reduction was a preparation for cell-fusion, instead of being the necessary consequence of cell-fusion.

A very curious case is that of *Fucus*, the Bladder-Wrack, which, like an animal, has only one colonial form—the familiar plant, and one protistoid reproduction, that producing the sexual cells; here, as we should anticipate, reduction occurs as in Metazoa at the inception of the latter process. Had this case been worked out before that of the Vascular Cryptogams, it would have afforded great support to the physiological hypothesis.

Again, the little fresh-water Algae, the Conjugatae, have their cells isolated, or at most in simple colonies of filaments, where the cells, placed end to end in a single row, divide each on its own account, so that they are really rather protistoid than comparable with the differentiated colonial cells of higher plants. In these plants nuclear reduction occurs at yet another point of the cycle, namely, at the very first cell-divisions of the zygospore, which is, as we see, the resumption of protistoid cell division after conjugation.

Strasburger's statement of this explanation is somewhat different. He writes: "The morphological cause of the reduction in number of the chromosomes . . . is in my opinion phylogenetic. I look upon these facts as indicating a return to the original

[1] I pass by exceptional cases where the reduction occurs at a very early period in the cells of the ovary.

generation from which, after it had attained sexual differentiation, offspring was developed having a double number of chromosomes . . . it is the re-appearance of the primitive number of chromosomes as it existed in the nuclei of the generation in which sexual differentiation [rather cell-fusion, for whether it be sexual or isogamous makes no difference to the point] first took place." If we are to take literally the phrases that I have spaced, we shall have to assume that two such plants as the onion and the turban-lily have independently developed a pairing process; for the number of the nuclear segments is 8 and 16 in the former, 12 and 24 in the latter; the same would apply to the two forms of the roundworm of the horse, with 2 (4) and 1 (2) segments respectively—which is absurd. Yet so much of the essay is taken up in proving that asexual reproduction is the older mode, not only in primitive organisms, but in individual Orders of higher organisms that one wonders if Strasburger has not really missed the inconceivability of his statement as it stands; and hence I cannot accord to the explanation above-given the full weight of his distinguished authority, as I should wish to do.

Now we have seen that the process of 'nuclear reduction,' despite its name, involves no necessary reduction in the quantity of nuclear matter, but only in the number of the segments into which it is distributed. Hence the process cannot have the physiological function ascribed to it as a 'preparation for gamogenesis'; and, since we have noted its occurrence at the inception of a long series of cell-multiplications, this physiological function would be absolutely useless.

II

A word about the functions of the chromatin or nuclein in nuclear division. The amount of chromatin in a nucleus is constantly changing; very often after a cell is formed the nuclein is much reduced in amount, and with this reduced amount the cell does all its individual life-work. At the approach, however, of cell-division, the nuclein grows, and reaches a maximum at the commencement of the nuclear division that precedes that of the cell as a whole; the nucleus of the daughter-cell repeats the conduct of its parent. Whatever be the function of the chromatin in the 'working' cell, as we may term it, it is evidently less important than its function in the dividing cell. The achromatic substance of the nucleus (linin) forms the basis, as it were, of the nuclear segments, the strands on which the chromatin is imbedded in the form of granules, like the string of a necklace, or better, the braid in beaded passementerie; these granules first split, and then the threads on which

they are strung. An explanation far removed from current theories has forced itself on me—perhaps after all it is the achromatic plasma ('linin') of the nucleus whose fair and equal division is the important matter, the final cause of karyokinesis. But the splitting of a viscid thread is one of the most difficult mechanical feats to accomplish. Suppose, then, that there is a certain polarity about the granules of chromatin, through which, after their division, they tend to recede from their fellows as far as possible; through this they will determine a splitting of the filament on which they are strung. The close of nuclear division sees their task accomplished; and, as we should expect, the chromatic granules having fulfilled this appointed task, now atrophy, and remain in this state till the approach of a new cell-division determines a fresh growth of their substance. According to this view the linin is the transmitter of inherited properties, and the chromatin has a purely mechanical function in karyokinesis. I may venture to predict that this hypothesis will be shortly incorporated into the newest edition of the germ-plasm theories; for it avoids the many difficulties due to the ascription of hereditary constancy to a substance so subject to periodic atrophy and growth as the chromatin of the nucleus.

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SOME NEW BOOKS

Sir William Flower's Essays

Essays on Museums and other Subjects connected with Natural History. By Sir William Flower, K.C.B., D.C.L., etc. 8vo, xv+394 pp. London: Macmillan & Co., 1898. Price, 12s. net.

In this volume are collected together a number of selected essays upon a variety of subjects of biological interest, extending over a period of years between 1870-1897, and, although all or nearly all have been published previously elsewhere and so rendered accessible to students and others, everyone will welcome this new issue, under one cover, of an important series of essays. Sir William Flower has, in these pleasantly written chapters, clearly aimed at interesting and instructing the general reader as well as the man of science, and his happy knack of putting his facts before his readers in a very clear and simple manner should ensure the volume being widely read. Few people have taken more pains to promote in the public at large a healthy interest in biological science, whether in his writings or in his administration of the national Natural History Museum in Cromwell Road. The essays in this volume are not arranged chronologically but, more conveniently, according to subject, under four main headings.

Under the first heading are a series of essays upon Museums. In some respects this may be regarded as the most valuable portion of the volume. When we think of the extremely important part which museums play, and still more might play, in the advancement and dissemination of scientific knowledge, suggestions as to their proper administration from so distinguished an authority, cannot but carry great weight, and it is to be hoped that the valuable hints which abound in these essays may strike home, and be the means of improving the museums, not only of this country but also abroad. It is only within recent years that museum administrators have awakened to a sense of their responsibilities, and the awakening has even now not been general. In these chapters are many eminently practical suggestions as to the proper aims of museums and the methods which should be adopted. The suggested design of a one-storied building for a national natural history museum is at once simple and practical, though it entails as a *sine quâ non* a liberal allowance of ground space. The author's remarks relating to local museums and school museums should be seriously considered by the authorities at the head of such institutions. Were his advice followed the minor museums would become what so few are now, highly instructive—little educational centres, in fact. Each county museum might, by special attention to the systematic collection of objects of local interest, become of real value to specialists as well as to the casual

visitor. Progress, continuity, and a definite system or scheme of arrangement, should be ensured to every museum. Arrested growth or stagnation is fatal to any such institution.

The second division of the volume is entitled "General Biology," and under it are included eight essays upon a variety of subjects of biological interest. The doctrine of evolution supplies the main text of most of the lectures and addresses, and the theory and facts are dealt with in a simple, straightforward manner which will commend itself to the general reader, although the subject as discussed has now lost the charm of novelty which it possessed at the time when the essays were written. The history and progressive work of the Zoological Society affords one theme, and the account of one of our most successful societies, which has managed in the happiest manner to combine valuable scientific investigation with popular instruction, is very acceptable from the pen of the president of the Society. In some respects the two chapters which complete the biological section may claim to be the most interesting. In them the natural history of the Cetacea is discussed in some detail. This group of Mammalia has always been a favourite one with the author. One would have welcomed a more detailed comparative account of the various methods of pursuit and capture adopted in the different 'fisheries,' but the limits of time imposed on a lecture have not permitted this. These two essays, while giving an excellent résumé of the subject, point also to the serious incompleteness of our knowledge of this most interesting and specialised mammalian group. The habits alone of these animals offer a grand field of investigation to naturalists who have time and means at their disposal, our information relating to them being singularly defective. A vigorous research into the foetal development of some of the apparently more primitive species would be a work of great importance. The new gallery at the Natural History Museum, devoted to the Cetacea, and due to Sir William's own energy, should supply a stimulus to further active research.

Under "Anthropology" we find five essays, three being in the form of presidential addresses. The study of Man is dealt with from a general standpoint, and the history and present position of the science are gone into. The author lays much stress upon the importance of the comparative study of the various races of Man, and it is quite clear that long and laborious research will yet be necessary before we are in any position to lay down an even approximately satisfactory classification of the human species. The investigation is beset with difficulties, and even the terminology will require frequent revision. The classification is discussed on a primary basis of three main groups —the black, yellow, and white races—which have hitherto proved the most reliable wide divisions. Much of importance could be added to the scheme of classification as suggested in 1885, but the main points would be left unaltered. An interesting lecture on pygmy races deals with a number of, for the most part at any rate, very primitive peoples, more or less sharply marked off from the races among which they are situated, and from whom they keep aloof. Pygmy races enjoy a wide geographical distribution, though homologies can be traced in even widely separated groups, particularly amongst those referable to the 'black' primary race-division. The essay on "Fashion in Deformity,"

dealing with the many and varied instances of artificial deformation of the person for purposes of fashion, was published in book form ("Nature Series") in 1881, and has probably been very widely read. The customs of this nature are of even greater interest than might appear from a perusal of this article, since there is much to study in the motives which have led to the curious practice, the initiation and other ceremonies associated with many modes of deformation, the test of endurance for which some of them serve, and so forth. Interesting as is Sir William Flower's account as an introduction to the subject, it is high time that a comprehensive general work were compiled from the large mass of information which has now accumulated.

The volume concludes with short biographical memoirs on four great biologists—Rolleston, Owen, Huxley, and Darwin.

H. BALFOUR.

PACKARD'S ENTOMOLOGY

A TEXT-BOOK OF ENTOMOLOGY, including the Anatomy, Physiology, Embryology, and Metamorphoses of Insects; for use in agricultural and technical schools and colleges, as well as by the working entomologist. By Alpheus S. Packard, M.D., Ph.D., Professor of Zoology and Geology, Brown University. Pp. xviii. and 730. 1 plate and 654 figures in text. New York: The Macmillan Co. London: Macmillan & Co., 1898. Price, 18s. net.

THE title "Text-Book of Entomology" too often denotes a work comprising a meagre outline of the external morphology of insects, and a bare recognition that they possess some internal organs, followed by well-nigh interminable summaries of the characters of orders and families and catalogues of genera. The 'working entomologist' is too easily tempted to devote his whole attention to those outer structures of insects which enable him to classify their multitudinous species, and to neglect those vital organs whose form and development throw so much light on the most interesting problems of insect-life. Hence the 'text-book' is apt to be as dry as the specimens whereof it treats.

Prof. Packard's previous writings would lead us to expect from him a text-book of a very different kind, and students will not be disappointed with the present work. It is divided into three parts. The first deals with morphology and physiology; a short discussion on the place of insects in the animal kingdom, and their relation to other arthropods, is followed by nearly 200 pages devoted to external and 300 to internal anatomy and functions. The second part, consisting of 80 pages, contains a summary of our knowledge of insect embryology. The concluding 130 pages are occupied with an account of insect metamorphosis followed by some speculations as to the origin of the larval and pupal stages. There is no scheme of the classification of insects. A summary of the orders recognised by Prof. Packard would have been desirable, as there is, necessarily, frequent reference to ordinal names; but these will be understood by most who are likely to use the book, which is by no means an elementary treatise. The author does express the opinion that the Collembola and Thysanura are worthy to be separated from other insects as a distinct sub-class, a view hardly tenable when we consider the close similarity of such a springtail as *Japyx* to the wingless earwigs.

The introductory chapter on the relationship of insects to other arthropods is valuable, but it might have come better at the end of the

volume, when the student would have been in a position to attack the problems raised with fuller knowledge of the facts. Prof. Packard goes with the majority of those zoologists who discussed the arthropods some time ago in the pages of *Natural Science*, considering them a group of multiple origin which "may eventually be dismembered into, at least, three or four branches." In the "provisional genealogical tree," which "may serve to show in a tentative way the relations of the classes," the Trilobita are separated from the Crustacea and placed near the common ancestors of the Merostomata (including *Limulus*) and the Arachnida. Prof. Packard therefore rejects the division of arthropods into branchiates and tracheates, considering that the air-tubes of spiders and mites show no necessary relationship between those animals and the insects. *Peripatus* and *Scolopendrella* appear in the direct line of the ancestry of insects, the myriapod stem branching off below *Scolopendrella*. The Diplopoda and Chilopoda are given as separate classes in the tree, though in the text the author seems inclined to defend the old Myriapoda as a natural class against the views of Kingsley and Pocock. Most zoologists will be surprised to see that the millipedes are placed nearer to the insects than are the centipedes; Prof. Packard still attaches considerable importance to the six-legged larvae of *Julus* and *Pauropus*. There is a short account of the anatomy of *Peripatus;* in the points which separate it from the worms it is surprising to find no mention of its reduced coelome and secondarily-formed body-cavity. The description of *Scolopendrella* is fuller—a valuable summary for English readers of the recent researches of Haase, Grassi, and Schmidt on this very interesting creature with an original figure of its internal anatomy. But the statement that *Scolopendrella* "seems to be, like other archaic types, cosmopolitan in its distribution," is puzzling, since a restricted or discontinuous distribution is certainly characteristic of most archaic forms of life.

A synopsis of the characters of insects generally introduces the section of the book devoted to anatomy. The statement here that the labium is formed of the two *laciniae* of the second maxillae fused together seems a curious slip, and the saw-fly grubs are not mentioned among the larvae with functional abdominal limbs. In the detailed account of the external structure of insects, advantage is taken of the latest researches on the mouth-organs, attention being specially directed to the mandibles and first maxillae in the primitive Lepidoptera. A fuller treatment of the piercing and sucking mouth-organs of the Diptera and Hemiptera would have been desirable; the statement is made that functional mandibles are lacking in the latter order, but Prof. Packard does not say if he agrees with Lowne in regarding the lancets usually identified with those appendages as belonging to the maxillae. The epipharynx and hypopharynx are treated at considerable length—a valuable feature in the work, as these organs usually receive much less attention from entomologists than the paired jaws. The author states that Miall and Denny are mistaken in denying the presence of an epipharynx in the Orthoptera. He believes that the insect head consists of at least six segments, including the primitively pre-oral lobe which carries the eyes and ocelli, and the intercalary or tritocerebral lobe between

the antennae and mandibles. The outer skeleton of the thorax is described in detail, and the account of the structure and modifications of the legs and feet is specially full and suggestive, being accompanied by an excellent summary of the mechanics of insect limbs as used for walking, climbing, and swimming. This is mainly derived from Graber's work, supplemented by later researches. Like most American zoologists, Prof. Packard is inclined to allow much weight to the effect of use, disuse, and mechanical pressure in bringing about modifications in limbs. There is even reference to the strange opinion that the absence of tarsal segments from the front legs of certain digging-beetles is a character acquired by frequent mutilation and now become hereditary. In the account of the wings, there is no comparative view of the venation in various orders of insects. There is, however, a good summary of what is known of wing-development both in the lower and higher (metabolous) insects. Prof. Packard rejects the once fashionable theory that insect-wings are modified tracheal gills, believing that they originated in some purely terrestrial form; but he considers it likely that the folds of skin whence they were derived had originally a breathing-function. He agrees with the orthodox view that the elytra of beetles are modified front wings, rejecting their identification with the tegulae of Hymenoptera as suggested by Meinert and Hoffbauer. Not only the cerci and stylets, but also the paired external genital organs are regarded as true appendages of the abdomen. The section on the outer form of insects concludes with an account of such outgrowths of the skin as tubercles, hairs, spines, and scales, and a chapter on insect coloration, the latter dealing rather with the physical and chemical than with the bionomic aspects of that wide subject.

The portion of the book on the internal anatomy and physiology of insects commences with an account of the muscles, followed by a chapter on the nervous system, in which the author accepts Viallanes' threefold division of the insect brain, copying many of his figures, while a well-selected series of figures from Brandt illustrates stages in the fusion of the ganglia of the ventral chain. The histology of the nervous system is dismissed somewhat briefly. On the other hand the sense organs are fully dealt with, and a clear summary of the conflicting views of various naturalists on the method of insect vision, and the functions of dubious antennal structures is valuable; but there is only a mere mention of the ear in the basal antennal segment, described by Hurst and Child. The comparative anatomy of the digestive tract is full and well illustrated, and is followed by a short account of digestion and secretion. Then the salivary and spinning glands, urinary tubes, wax-glands, repugnatorial scent, and poison-glands are dealt with. The author is not inclined to follow Brauer in attaching importance to the number of urinary tubes as showing the relationships of the insect orders. The accounts of the circulatory, respiratory and reproductive systems are excellent; especial care is given to the minute structure of the breathing-tubes, the comparative morphology of the genital ducts, and the formation of the germ-cells.

The section on embryology is mainly drawn from the work of Korschelt and Heider, and is illustrated by copies of their figures, so that it forms a valuable introduction for English students to the

development of insects within the egg. But it is surprising to find no reference whatever to parthenogenesis; the fact that reproduction by virgin females is common in certain insects is now widely known, and some discussion of the subject might be expected even in a much more elementary work than this. Nor is there any mention (except incidentally on the last page of the book) of the fact—most suggestive in its bearing on the possible origin of degenerate groups—that some insects become sexually mature in the larval stage. It is hard to understand what considerations can have led to such omissions as these.

The concluding part of the book deals with metamorphosis, and contains a short summary of the external forms assumed by larvae and pupae, and of the development of the imago from the earlier stages. The structure of the typical larvae in the great orders is by no means fully described; but there is a more detailed description of those interesting forms which pass through what is called a "hypermetamorphosis." The formation of the adult organs in Lepidoptera and Hymenoptera from the imaginal discs is described in detail, while a special section is devoted to the corresponding phenomena in the Diptera which have been studied better than any other order in this connection. Prof. Packard considers the campodiform larva to be more primitive than the eruciform, and figures a series of beetle grubs which show the transition from the one to the other. In a final chapter the fascinating question of the origin of metamorphosis is discussed; and the author comes to the conclusion that the transformations which now characterise all the higher insects have been acquired since the group obtained the power of flight.

A very valuable feature of the book is the full bibliography appended to each section; by means of this the student will be enabled to follow up any subject on which he desires further information. The figures are numerous, and as a whole good. Though the execution is, in some cases, rough, comparing unfavourably with the beautiful drawings in the Cambridge Natural History, for instance, only a few, such as the larva of *Eristalis* on p. 430, can be considered unworthy of the book. Altogether the work supplies a long felt want, and all serious students of insects should be grateful to Prof. Packard for having given them in a single volume so full a summary of what is known about insect structure and life-history.

GEO. H. CARPENTER.

ESSEX VERTEBRATES

THE MAMMALS, REPTILES, AND FISHES OF ESSEX: a contribution to the Natural History of the County (Essex Field Club Special Memoirs, Vol. III.). By Henry Laver, M.R.C.S., &c. 8vo, pp. 1-138, with 8 full-page illustrations. Chelmsford: E. Durrant & Co.; London: Simpkin, Marshall & Co.

FOR many years the attention of those fortunate naturalists who live in the country and have opportunity to observe nature at home has been largely devoted to birds, as is indeed natural from the readiness with which those creatures are seen and studied, and their own extreme attractiveness. It thus happens that while nearly every county in the British Isles possesses a popular work on its birds, the other vertebrates have been very largely neglected.

This is the more to be regretted, seeing that while the occurrence of birds in particular areas must, in many cases, be a mere matter of chance, the presence of other non-volant vertebrates, depending as it does on quality of soil or water, temperature, dampness or dryness, open country or forest, flatness or hilliness, will often illustrate genuine differences in the natural characters of the areas treated of. Therefore, working up from county faunas, we may hope to see in time a scientific classification of British faunal areas, a work which can only be done satisfactorily when the terrestrial vertebrates have been very much more studied than is now the case.

The book before us deals with the vertebrates, other than birds, of the county of Essex; and we may congratulate the Field Club of the county on the charming little work its vice-president, Mr Laver, has produced under its auspices, and may hope that so attractive a book may influence other Essex naturalists to take up the study of the groups it treats of.

According to the author's present knowledge Essex possesses 38 terrestrial and 10 marine mammals, 4 reptiles, 6 amphibians, and 113 fishes, but he expects this number to be considerably increased so far as the marine mammals and fish are concerned.

More than half the book is devoted to the mammals, and Mr Laver has given us a number of interesting notes on the habits and local distribution of the smaller members of the class, as yet so insufficiently studied from the field-naturalist's point of view. This seems to be the most original part of the work, not depending, as so much of the remainder necessarily does, on 'records,' but on the author's personal observations, and the qualities here shown lead us to hope that we may see further contributions from his pen in this direction.

In his nomenclature Mr Laver has wisely followed Mr Boulenger for the lower vertebrates; but in the mammals, with a certain perversity, he tells us that he has accepted the rather out of date Bell and Southwell for the seals and cetaceans, while he has consulted our greatest authority on those very groups, Sir W. Flower, for the Carnivora, Rodentia, and ungulates, with whose nomenclature Sir William has seldom had need to trouble himself. It is not Mr Laver's fault that his book was written just before Mr Miller's researches caused such a *bouleversement* in the nomenclature of our bats, but we may hope that in any future contributions from him a more modern system of nomenclature may be followed.

A last word of commendation must be said for the printing, get-up, and arrangement of the book; while many of Mr Henry A. Cole's illustrations—notably the "Badger Earth, Epping Forest" (p. 42)—are quite charming. O. T.

Wiedersheim's Anatomy of Vertebrates

Grundriss der Vergleichenden Anatomie der Wirbelthiere. By Dr Robert Wiedersheim. Fourth Revised Edition. Jena: G. Fischer, 1898. 8vo, pp. xxiv, 560. Price, unbound, 14 Marks; bound, 16 Marks.

Both students and teachers of the anatomy of vertebrates will welcome the fourth edition of Prof. Wiedersheim's well-known "Grundriss." Although the present volume is smaller than that

of the preceding edition (1893), it is still somewhat unwieldy for a "Grundriss." The original "Grundriss" (1884) was a small octavo volume of 272 pages, and represented an abridged edition of the "Lehrbuch," but it has now increased so greatly in size as practically to have replaced the "Lehrbuch" altogether, for no new edition of the latter work has appeared since 1886.

One of the special features of the last two editions of the "Grundriss"—a feature which primarily distinguished the "Lehrbuch"—is the extensive and valuable bibliography, occupying more than a hundred pages. This commends itself strongly to the advanced student, and, the titles being classified according to the organs or systems of organs dealt with in the memoirs, reference to previous literature is greatly facilitated. One of the disadvantages of the method, however, arises from the fact that many papers treating of several organs demand quotation under a number of headings, and this in the book under consideration has, probably to economise space, not always been accorded. Thus, while Huxley's well-known paper on *Ceratodus* is quoted, as it should be, under the heading "Freie Gliedmassen," it does not find a place under "Schädel der Fische," although it contains most valuable information upon the skulls of *Ceratodus*, *Cestracion*, and *Notidanus*. Numerous other instances might be given, but as it is not possible to suggest a remedy without adding too much to the bulk of the book (except, perhaps, by some system of cross-references) we must be grateful for having our attention directed to even a few papers which in the ordinary course of work might be overlooked.

The profusion of illustrations which invariably characterises the works of Prof. Wiedersheim, and the subordination of the taxonomic to the physiological classification in the arrangement of the chapters, are features which render the book attractive and interesting even to the beginner. It does not follow, however, that a ready sale will be found for the book among English students, for in scope and bulk the present work is almost identical with Prof. W. N. Parker's second English edition, 1897, founded on the third edition of the "Grundriss."

There is abundant evidence of careful editing in the volume under consideration, but entire freedom from mistakes cannot be admitted. *Caudalvenen* appears for *Cardinalvenen* (p. 368, last line but two), and *Metapterygoid* for *Metapterygium* (fig. 96, p. 106), while in fig. 232, p. 275, the letters A and B are transposed. The *ductus endolymphaticus* (fig. 214, p. 252) appears, as in the earlier editions, on the external instead of the mesial surface of the labyrinth, and the side view of the skull of the greyhound (fig. 88 B, p. 96) exhibits four upper incisor teeth; but such blemishes as these are happily few, and the new figures are, on the whole, very good. W. G. R.

The Histology of Vertebrates

LEHRBUCH DER VERGLEICHENDEN MIKROSKOPISCHEN ANATOMIE DER WIRBELTIERE. II. SCHLUND UND DARM. By Prof. Dr Med. Albert Oppel. 8vo, pp. viii + 682, 4 plates and 343 text-figures. Jena: G. Fischer. 1897. Price, 20 marks.

THE second part of Dr Oppel's laborious task carries out the promise of the first. There is the same wealth of detail culled laboriously from multifarious sources and expanded by original matter, and the same careful citation of authorities. The author begins with an

account of the main features of the macroscopic structure of the alimentary canal in the various groups of vertebrates and then enters upon a detailed account of the oesophagus. This occupies the first 160 pages of the volume, and is perhaps remarkable chiefly on account of the close similarity of the structures to be found in different vertebrates. With the exception of the peculiar elaborations forming the crop of birds, there is not any marked difference to be found in the various orders.

In the treatment of the gut the greater variety of structure demands a fuller sub-division of the matter. The systematic treatment has to be repeated for a whole series of structures such as the epithelium, the musculature, the various forms of glands, the lymph-tissues, blood-vessels, and nerves. This occupies the volume up to page 537, and the remaining portion treats of the development and of special structures such as the caecal diverticula and of all such remains. There are copious indices of matter, authorities, and animals. We welcome a scholarly and careful contribution to anatomy.

Ornithologists at Dresden

Aus der 22. Jahresversammlung der Deutschen Ornithologischen Gesellschaft in Dresden, vom 28-30 Mai 1897, herausgegeben von A. B. Meyer. *Abh. K. Zool. Anthrop. Mus. Dresden*, vii. (2). Berlin: R. Friedländer & Sohn, 1898. 4to, pp. vii. 83, pls. i.-iii.

Dr A. B. Meyer has rendered useful service in editing the papers read at the Dresden meeting of German ornithologists. He is himself responsible for a treatise upon the *Paradiseidae*, extending over some thirty-five pages, in which he refers to several of the recent discoveries in this important family, which has of late yielded so many unexpected novelties to collectors. Dr Sharpe and Mr Hartert have already shown a special knowledge of the subject; but it is satisfactory to find our continental colleagues assisting in elucidating the difficult questions which these birds present to the student. It is also pleasant to learn that the Dresden Museum possesses specimens of a good many Birds of Paradise, including several of the original types. The first appendix to this paper supplies a useful list of recent publications dealing with the *Paradiseidae*. Many naturalists will be surprised to learn that we are now acquainted with the eggs of no less than twelve species of Birds of Paradise.

Of more general interest than the foregoing is the charming sketch of bird-life upon the Nile contributed by Dr Koenig of Bonn. Englishmen have of course long since worked out the general features of the ornithology of Egypt; but while our countrymen continue to supplement our local knowledge—witness Mr Cavendish's recent recognition of *Gyps rueppelli* in the Cairo Zoological Gardens, and the capture of the fourth known example of *Saxicola xanthoprymna* by the same veteran 'Ibis,'—there is plenty of scope for other workers, and Dr Koenig's industry is acceptable. His present paper only covers about thirteen pages, but it is brimful of facts and very pleasant reading. Dr Koenig paid special attention to the species of Hirundinidae to be met with in Egypt, including *Hirundo savignyi*, which he naturally considers to be a local and resident form of the more widely distributed *Hirundo rustica*.

An excellent coloured plate reproduces no fewer than three recently discovered sketches of the lost Dodo, or 'Dronte,' as the

Germans elect to style it. Two of these small sketches exist in the Dresden picture gallery. It should also be remarked, that Von Biedermann gives a list of the known representations of the Dodo, numbering fourteen altogether.

Dr Meyer pens a short note upon remains of *Alca impennis* from Swedish deposits, including a left coracoid bone obtained at Greby in the province of Bohuslän, among fragments of pottery and bones of the ox, pig, and sheep.

Finally, we must draw attention to the essay of Mr Voigt upon the love-notes of the Capercailzie or 'Auerhahn' (*Tetrao urogallus*), and the Blackcock or 'Birkhahn' (*Lyrurus tetrix*), which he has taken the pains to set to musical notation; and to a remarkable variety of the common European Kingfisher (*Alcedo ispida*), which is admirably figured. The curious point about this specimen, which was procured upon the Rhine between Mainz and Worms, is that the plumage of the upper surface is half green and half deep blue, the two colours being equally distributed. H. A. M.

Sponge Spicules

Materials for a Monograph of the Ascons. I. On the Origin and Growth of the Triradiate and Quadriradiate Spicules in the Family Clathrinidae. By E. A. Minchin, M.A., Fellow of Merton College, Oxford. Reprinted from the *Quarterly Journal of Microscopical Science*, n.s. vol. xl., pp. 469-587, pls. xxxviii.-xlii., January 1898.

Mr Minchin is almost a social phenomenon. He was appointed nearly five years ago to an Oxford fellowship, after competitive examination in natural science. The formal theory of such fellowships is that they should fill, so to speak, the yolk-sac of an embryonic scholar in some branch of human knowledge, and provide him with the metabolic material known as gold. These halcyon years the fellow (theoretically) devotes to the pursuit of his particular branch of knowledge; he has the opportunity to develop into a fully equipped investigator, and may be supposed in this process to have issued a considerable body of actual research. At the end of that period, on the aboriginal hypothesis as to the place of fellows in the universe, a patron should present the fellow to a fat country living, where, dwelling in the fear of God and the friendship of the squire, the endowed scholar should continue his studies. In the modern world, however, a fat country living is not the natural reward of successful investigation. For this reason it happens, at least in Oxford, that the research fellow seldom does research. Sometimes he goes to the bar; sometimes to medicine; sometimes to commerce; sometimes he enters with ardour into the boarding-school industry, and becomes a useful man to his college. In any event it is almost an anachronism for him to devote the major part of his endowed time to research. How Mr Minchin can reconcile it with his own interests that he has fulfilled the plain purpose of a fellowship by giving up his time to research we cannot pretend to say, but the first part of the monograph now before us, and a series of earlier memoirs, are ample evidence as to the fact.

The present memoir deals with points incidentally obtained in the course of Mr Minchin's attempt to get materials for a complete monograph of the Ascons. For all sponge histology he found it of importance to carry with him, when seeking for specimens, tubes containing

the necessary reagents, and so to secure preparations killed in an absolutely fresh condition. For study of the spicules he used a one per cent. osmic solution diluted with an equal volume of sea-water. After five or ten minutes in this, the specimens were rinsed in water and placed in picrocarmine, in which they were allowed to remain for one or two hours, and were then transferred to glycerine or to alcohol, according as surface-views or sections were required. For spicules he found this method more useful than nuclear stains, as these, from their acidity, corroded the crystals. Certainly the exactness of the results gained, as shown in the description and Mr Minchin's exquisitely beautiful drawings, is proof of the excellence of the method.

After exact description of the spicules and their mode of formation in a series of types, the author passes to a general review of the nature and condition of the spicule systems. He lays considerable stress on the presence of the spicule sheath between the spicules and their secreting cells, and cannot follow the view that spicules are formed in protoplasmic nodes, and owe part of their structure to resulting tensions. He regards the sheath as the remnant of a vacuole secreted by the cell, in the interior of which the spicule itself appeared as a concretion. The primordial form of spicule he takes to be a simple monaxon: the triradiate type he regards as being formed of three monaxons fused at a point. Probably the original monaxons were arranged as the sides of hexagon figures surrounding the pores. It has been objected to a composite origin of the triradiate spicules, that these behave optically as single crystals; yet Mr Minchin shows that in actual ontogeny they arise from the very early fusion of these. The quadriradiate crystals he supports Haeckel in supposing to be derived from triradiate forms by the addition of a gastral ray.

In the case of a memoir like this, which depends in every way upon the exposition of elaborate details, it is impossible to do justice in a short notice. We can only commend it to our readers as a striking and elaborate piece of work, and to the University of Oxford as a singular result of the activity of that supposed rudimentary organ—the endowment of research by fellowships.

The Life Study of Buchanan White

The Flora of Perthshire. By Francis Buchanan W. White, M.D. Edited, with life of Author, by James W. H. Traill. 8vo, pp. lx + 408, portrait and map. Edinburgh: Printed for the Perthshire Society of Natural Science by W. Blackwood & Sons. 1898. Price, 7s. 6d.

This handsome and well-printed volume has a melancholy interest to all lovers of British Botany. Dr Buchanan White died in 1894. How much Perthshire Botany owes to his continued labours in the cause of science can only be gathered from the study of this volume. He founded the local Society. He had a large share in raising the Perth Museum to its present position of supremacy amongst those in county towns. The catalogue of his printed works in this volume contains about 227 headings. Moreover although the editor has been obliged to obtain the assistance of others in some of the larger more critical genera such as *Hieracium* (and curiously enough *Salix*), which were not written up by Dr White, still the work is to all intents and purposes his work, and its details and accuracy are exactly what one would have expected from him.

The book contains a portrait of the author, a map of Perthshire, and Prof. Traill has also contributed a memoir, a list of scientific papers, and an introduction. Obviously Dr White's own introduction (43 pp.) is incomplete. Apparently it has been thought best to publish it, so far as possible, without alteration, though some minor changes have been introduced. Hence when the heading "Geology of Perthshire more especially in its relation to the distribution of the Flora" arouses pleasant anticipations of an account of a sorely neglected part of field-botany by one well qualified to judge, we must not be disappointed to find nothing whatever about the Flora, only geological information and a few notes as to the fertility of the soils.

The subdivisions of the county are based on the river-valleys. Fortunately we have a Highland and a Lowland Isla as well as similar divisions for Perth, Earn, and Forth. Most unfortunately the vicious system of Watsonian vicecounties has not been entirely thrown away and this is the solitary fault to be found with this handsome volume. There is in this Flora, as indeed in most county Floras, nothing to show that Darwin or Drude or Engler or Warming or the numerous tribe of German, Scandinavian, and Russian botanists ever existed. The ideas of distribution in this country have stopped with Mr H. C. Watson. If the distribution alone has to be made clear, one might have thought that Mr C. B. Clarke's paper on Tabulation Sub-areas had proved clearly enough that the only satisfactory plan is to make subdivisions based on degrees of latitude and longitude. If these are drawn small enough, any required degree of accuracy in the range of a species can be obtained, and its distribution on a railway line, in a valley or along a mountain chain stands out clearly. For the study of plant-associations or plant distribution on modern lines, this book affords no help whatever. Fortunately the absurd West Perth, East Perth, and Mid Perth have been disregarded. Dr White has gone so far as to separate the Old Red Sandstone districts from the Pre-Cambrian or "Silurian" (p. 4) and this is the line of division followed between the Highland and Lowland Earn, &c. Presumably, like many others, he was afraid to disturb the faith of the average British botanist in the late Mr H. C. Watson.

The actual treatment is best seen by a typical example.

ORDER XXXIX.—ERICACEAE.

TRIBE I.—ARBUTEAE.

1. **ARCTOSTAPHYLOS Adans.**

A. Uva-ursi Spreng. (87, 88, 89.)

L. O Earn, O O O

H. O Earn, Perth, Isla, O Breadalbane, Rannoch, Atholl.

HAB. Dry rocky places on the mountains. Local.

ALT. Ascends to 2350 ft. in Rannoch and 2000 in H. Earn and Atholl.

Loch Rannoch (Professor Hope, 1762).

Almost confined to the Highland area; but near Crieff it occurs on conglomerate rocks of the Old Red Sandstone.

L. stands for the Lowland Districts on Old Red Sandstone. H. for the Highland on older rocks. "O" means that the plant does not

occur in the district which comes in this position, *i.e.*, in the above case, the plant is absent in lowland Perth, Isla, and Forth, as well as in Gowrie.

But in some cases there is also a short and much needed account of the less known varieties, *e.g.*, *Trifolium dubium*, Sibth. var. *pygmaeum* Soy.-Will. is described as follows, "Dwarfer; petiole of middle leaflet not longer than the lateral ones." These occasional deviations from the plan are probably the most valuable part of the book, and we wish there had been more of them.

Mr F. J. Hanbury has edited the Hieracia, Mr H. Groves has worked through the Characeae, and Messrs W. Barclay and H. Coates have afforded Prof. Traill much assistance with those genera (*Salix*, *Festuca*, etc.), which were not finished in the MS. Prof. Traill's portion of the work must have been exceedingly difficult; he has however succeeded in presenting Dr Buchanan White's work in the best form possible under the circumstances, and deserves congratulation on the accomplishment. The map (by Bartholomew) is very good, coloured according to altitudes of 500, 1000, 2000, and 3000 feet; but is on far too small a scale for detailed work in the field.

The Ferns of the Earth

Die Farnkräuter der Erde. Beschreibende Darstellung der Geschlechter und wichtigeren Arten der Farnpflanzen mit besonderer Berücksichtigung der exotischen. Von Dr H. Christ (Basel). 8vo, pp. xii+388, with 292 figures. Jena: Fischer, 1897. Price, 12 Marks.

In this work Mr Fischer has added another useful book to his long list of botanical publications. Apart from their general life-history the modern botanist knows next to nothing about ferns, and is often fain to admit his ignorance of the names of the most commonly cultivated species. He knows *Pteris aquilina*, the bracken fern, but is surprised to find that a fern which is used for decorative purposes, along with one or more equally unknown palms, in most of the London dairies, or in smaller editions as an ornament for the dinner table, is congeneric. He will find Dr Christ's book very useful. It is not a complete monograph, for such a work would require several volumes of equal size to the one now before us, and it is therefore not intended primarily for the specialist, but notwithstanding includes descriptions of 1154 species belonging to 99 genera. If further information is required, it can be found in one or other of the systematic works of which the author gives a list at the beginning of his book, or of the floras which follow arranged in geographical sequence. Under the title 'Ferns' Dr Christ includes the so-called true Ferns or Filicinae of Prantl which fall into two groups, namely, the Leptosporangiatae, in which the spore-cases are derived from a single (epidermal) layer of cells, and the smaller or Eusporangiatae where several cell-layers are concerned. The first group contains the Hymenophyllaceae, Polypodiaceae, Davalliaceae, Osmundaceae, Cyatheaceae (tree-ferns) and others, the second the two small families Marattiaceae and Ophioglossaceae. The first part of the book (13 pages) is a brief systematic outline of the characters of the groups and genera. The remainder, forming Part II., is a more exhaustive account of these and of the principal species, including all well-established species, "which are in any way remarkable on account of

structure, biological peculiarities, phylogenetic relations, and wide or peculiar distribution." The descriptions are all in German, and those of individual species are remarkably non-technical and lucid. In each case the native habitat is given. The illustrations, which are numbered to correspond with the species depicted, if not of a very high order, are at any rate good enough to be an efficient help in elucidating the text, and assisting in the determination of the specific name. The book concludes with a good index.

The Reproduction of Plants

Beiträge zur Lehre von der Fortpflanzung der Gewächse. By Prof. Dr M. Möbius. 8vo, pp. viii., 212, with 36 figures in the text. Jena: Fischer, 1897. Price, 4 Marks 50 pf.

Prof. Möbius has here brought together the matter contained in several essays previously published in the *Biologische Centralblatt* (1891, 1892, and 1896), which, with some additional chapters, make up a useful and suggestive discussion of the methods of reproduction in plants, both vegetative and sexual, the conditions on which they depend, and the relations between the two kinds. There are five chapters, namely:—1. Introduction. 2. On the consequences of continued vegetative multiplication of plants. 3. On the conditions that govern the flowering of plants. 4. On the relation between spore and bud formation in the reproduction of plants. 5. On the origin and significance of sexual reproduction in the plant-world. In his introduction the author emphasises the contrast between the life of the individual and the life of the species, as the key to the relation between vegetative and spore reproduction, and indicates how the sexual process may have arisen as a secondary result in the latter case. In the second chapter a number of instances are adduced in support of the contention that senile decay, as a result of continued asexual reproduction, exists "only in the imagination of certain authors and breeders." Many cultivated plants, like the sugar-cane, have been propagated from time immemorial only by the vegetative method; many, in fact, can be multiplied in no other way, and there is no evidence to show that these are weaker or less healthy than the offspring of a sexual process. They have no predisposition to disease. Epidemics are not peculiar to vegetatively propagated plants, but appear even among wild plants, both annuals and perennials. An important feature of the book as a whole is the great number of examples which the author adduces in support of his contentions, rendering it useful to the student quite apart from the intrinsic value of the conclusions based thereon.

The Makers of Andrée's Balloon

Andrée and his Balloon. By Henri Lachambre and Alexis Machuron. 8vo, viii + 306 pp., coloured frontispiece, and 44 full-page illustrations. London: Constable & Co., 1898. Price, 6s.

The main interest of this work arises from its detailed description of Andrée's balloon. Whatever geographical results we may learn from Andrée when he returns in the autumn, as there are still good grounds for hoping that he will, his expedition has already done valuable service by its contributions to aeronautics. Andrée, as an

expert mechanician, has introduced many novelties into ballooning equipment. The senior author of the work is Mr Henri Lachambre, the well-known French balloon-maker, who made Andrée's balloon and went to Spitsbergen in 1896 to superintend its installation and inflation. This work was successfully achieved, but the weather was unfavourable and no start could be attempted that year. Few things show better Andrée's courage and good judgment than his determination not to risk an ascent after the first few days of August. Even if he could then have relied on a strong, steady, south wind to carry him to the pole, and a continuation of it on the other side to blow him on either in Siberia or North America, he would have landed too late to make adequate preparations for the winter. Andrée accordingly faced a certain amount of scoffing by returning to Europe in 1896, ready for an earlier start in the following season. Mr Lachambre did not offer to go north again. One summer's exile on the ice-bound shores of Dane's Gat was enough for him. So it fell to the lot of his nephew, Alexis Machuron, to superintend the balloon work in 1897, and to see the actual ascent.

Mr Lachambre's account of his experiences in Spitzbergen is a quaint addition to Spitzbergen literature. He makes numerous observations on the fauna and flora, but his knowledge of natural history is too limited to render these of any value. He says, for example, that the auk is the same bird as the fulmar petrel, and then calls it a duck. As an example of his remarks on the vegetation we may quote the following: "There is no vegetation to gladden our sight, nothing but a few varieties of moss bearing tiny white, violet, and yellow flowers; the yellow ones, larger than the rest, resemble very much the buttercups with which our meadows are dotted in spring." The flower-bearing "mosses" in question are no doubt species of *Ranunculus*, the Arctic poppy (*Papaver medicaule*), and rock roses. The translator by always speaking of eider-geese, for example, helps to caricature the natural history notes in the volume. The photographic illustrations are admirable, and help to atone for the numerous imperfections of the text.

A New Means of Research

Practical Radiography. By A. W. Isenthal and H. Snowden Ward. Second edition. 8vo, 157 pp. London: Published for *The Photogram* by Dawbarn & Ward. 1898. Price, 2s. 6d. net.

We welcome the appearance of this thoroughly practical little book. Now that the popular excitement due to the novelty of Dr Roentgen's discovery is abating, the real workers, those who are earnestly making use of the power placed in their hands, are coming to the front. That good work is being done is very clear from a perusal of the book. This is particularly marked in its application to surgery and medicine; and we are glad to find that one of its first uses has been to the alleviation of human suffering, one great end of scientific research. But there are many other fields where the new power will be of great value. Apart from the physician, men of science interested in Roentgen rays may be divided into two classes: those physicists who are investigating the nature and properties of the rays, and those who are occupied, not so much with the discovery itself, as with its application as a

means of investigation in their own special field of work. Foremost among these are the biologists, and their appreciation of the discovery is evidenced by the recent publication of the beautiful series of radiograms of the British Echinoderms by Prof. R. N. Wolfenden, the British Batrachians and Reptiles by Messrs J. Green and J. H. Gardiner, and many others. We feel sure that the peculiar power of this means of research need only be more fully known to become very largely used.

Returning to the work before us, there are one or two details that we must touch upon. The "Historical Review" is both interesting and comprehensive, but we scarcely think Prof. Roentgen has been given the prominent place due to him; he has certainly done more than to apply to practical uses a series of investigations begun by others; in fact, application to practical uses is just what Prof. Roentgen has not touched upon. We also note the term, 'Crooke's tube'; this is possibly a misprint, but should be Crookes' tube. Also, mention is made of a Rhumkorff (? Ruhmkorff) or Apps' coil; we fail to see the distinction. In chapter ii. (Apparatus), a quantity of very elementary matter is introduced, of which we doubt the utility, for it is not possible, in the space that can be devoted to it, to teach the elements of electricity; the same must be said of the early part of chapter iv. on photography.

The various forms of tubes which have been used for the generation of Roentgen rays are very fully illustrated, and are of considerable interest, as also is the description of the new Tesla oscillator and the more recent form of Ruhmkorff coil introduced by Messrs Rochefort and Wydts; in these particulars the book is thoroughly up to date. The illustrations, particularly the half-tone radiograms, are very good; but it is almost a pity that these latter are scattered about the book without any particular reference to the text. Chapters v., vi., and vii., on medical radiography, show the advance that has been made in this direction within the last two years. The book concludes with a concise and well-written *résumé* of the various theories that have been advanced as to the nature of the rays. On the whole, we can congratulate the authors upon the masterly way in which they have dealt with the subject, and wish the book a wide circulation.

"Science is Measurement"

Notes on Observations, being an Outline of the Methods used for determining the Meaning and Value of Quantitative Observations and Experiments in Physics and Chemistry, and for reducing the Results obtained. By Sydney Lupton, M.A. 8vo, pp. x + 126. London: Macmillan & Co. 1898. Price, 3s. 6d.

Although, as indicated in the sub-title, this little book is distinctly intended for the physicist and chemist, still, if we mistake not, it will be of considerable value to the biologist, and, indeed, to students of other branches of natural science. Although there are biologists among us who make much use of mathematical methods in their researches, and although there are mathematicians who love to trespass on the domain of the biologist, yet mathematics are, as a rule, somewhat of a bugbear to the ordinary student of biology, who, it is probable, spent the time he should have devoted to them in running after butterflies or exploring the inside of a frog. For the ordinary biologist

who desires to comprehend something of the work now being done on variation, on selection, on specific differentiae, and so forth, or who may even be bold enough to attempt some of these mathematical methods himself, Mr Lupton's book will come as a godsend. The chapters on Ideas, on Reasoning, on Fallacies, on Laws of Nature, on Cause and Effect, and on Observations and Experiments, with their references to larger works, may be read with profit by many who think they were taught all about such things at school. But it is when we get to the explanation of such things as Units and Dimensions, Averages, Differences, Mensuration, the Use of Tables, Errors, Means, the Law of Error, and Graphical Methods, that the special value of Mr Lupton's book becomes apparent. There are many who will be glad to learn the difference between an Average and a Mean, and more particularly to know which of the various Means is the one now selected by mathematicians as the Mean, while the difference between the Mean and the Mode is another subject upon which light is needed by a good many who are not so well acquainted with Prof. Karl Pearson's writings as they ought to be.

Nevertheless, valuable though Mr Lupton's book is, we should greatly like to see a manual on the same lines, but more especially adapted for the biologist. Nearly all Mr Lupton's are taken, as is natural, from physics or chemistry; Prof. Weldon and his crabs are not so much as alluded to. But the construction of graphical curves to represent biological observations presents difficulties and complications of its own. The useful application of logarithmically ruled section-paper was recently suggested by Mr D. J. Scourfield; while readers who are interested in the determination of species will find an interesting application of mathematical methods suggested by Prof. C. B. Davenport in *Science* for May 20. We merely mention these papers as examples of the kind of thing that might well be incorporated in a text-book of biological mathematics, and we can but regret that so useful a person as Mr Lupton has been lost to our own branch of science.

Historical Specimens of Fossil Cephalopoda

List of the Types and Figured Specimens of Fossil Cephalopoda in the British Museum (Natural History). By G. C. Crick. 8vo, boards, pp. vi + 104. London. 1898. Price, 2s. 6d.

To the serious worker who is called upon to identify organisms nothing is more helpful, and in critical cases essential, than a comparison with the specimen on which the original description was based; and to such persons there is no more welcome aid than a directory of these so-called type-specimens. As regards the British fossils, a Committee of the British Association has long been collecting information with a view to such a list, and owing to its exertions several museums (among others, those of Cambridge, Manchester, and York) have printed and published lists of their type fossils.

The present catalogue enumerates somewhere about 1000 specimens (a striking illustration of the wealth of the British Museum) with full references to the places where they have been described and figured, as well as to their locality and stratigraphical horizon.

They are arranged alphabetically according to the generic names

under which they were first recorded, but there is an index of specific names at the end which renders it easy to find those which have been subsequently placed in other genera. It is not always easy to distinguish with certainty between type-specimens and those which have subsequently been figured; and it was hardly to be expected that any distinction should be drawn between holotypes, cotypes, paratypes, and the like, useful though such distinction is to the systematist. We cannot help thinking that where a heading is repeated, it is a help to the eye to substitute a dash for the word rather than to reprint it each time. There is an error (probably merely clerical) in the account of *Goniatites iris* on p. 57.

Examination Zoology

Text-Book of Zoology. By H. G. Wells, B.Sc., Lond., and A. M. Davies, B.Sc., Lond. University Tutorial Series. 8vo, pp. 366. London: W. B. Clive, 1898. Price, 6s. 6d.

This is a new edition of the text-book prepared some years ago by Mr Wells for the University Correspondence College Press. The original book had many serious disadvantages; the present volume, all of which save one chapter, has been re-written by Mr Davies, is immensely improved. The descriptions are lucid and correct, and the diagrams very useful. We have gone over the volume with close attention, and with no particular favour for the Correspondence College system, but we have confidence in saying that a student who has not the opportunity of attending a regular course of zoology, will gain clear and correct conceptions from this text-book. It will be necessary, of course, for him to go through a considerable practical training in addition, and the authors of the volume constantly impress this upon his notice.

The Metric System

Now that the metric system of weights and measures is legalised in the United Kingdom, everything that tends to encourage its use is to be welcomed. The *Pharmaceutical Journal* has done good service in publishing tables of the metric equivalents of various imperial weights and measures, together with thermometric equivalents in degrees Centigrade, Fahrenheit, and Réaumur. These have now been reprinted as a quarto pamphlet, printed on one side of the paper only, and to be obtained at the *Pharmaceutical Journal* office, 5 Serle Street, Lincoln's Inn, W.C., price 1s. 6d. nett.

Zoology in Japan

All visitors to Japan know the Japanese Cicadidae. There are at least sixteen species found in Japan, of which nine are peculiar to that country. A systematic summary of these species, with the description of a new one, has been published by Mr M. Matsumura in *Annotationes Zoologicae Japonenses* (vol. ii., pp. 1-20, pl. i.). In the same number, Mr A. Iizuka describes a new species of littoral Oligochaete, under the name *Pontodrilus matsushimensis*, since it was discovered in Matsushima Bay, burrowing in the sand under the half-decayed leaves of *Zostera marina*. The species does not agree with Mr Beddard's definition of the genus, in that the *vas deferens* opens

in the same way as it does in the genera *Moniligaster* and *Ilyodrilus*, which belong to other families. Mr Iizuka suggests that this difference is due rather to ignorance of the true structure of *Pontodrilus* than to any divergence in the case of the present species. The last paper, by Mr M. Namiye, describes the indigenous snake *Achalinus spinalis*.

Bibliography

The Geological Survey of Belgium, having with some success published vol. i. of *Bibliographia Geologica*, specifying all works and papers on geological subjects that appeared during 1896, now proposes to issue a retrospective series. The first volume will comprise the titles of all geological publications in the library of the Belgian Survey, arranged according to their subject-matter on the plan of the decimal classification. Authors who wish to see their works quoted in so useful a catalogue will, it is hoped, send copies to the library in question. They may address the Director, Dr M. Mourlon, 2 Rue Latérale, Brussels.

New Serial

We welcome an Italian rival, *Rivista di scienza biologiche*, edited by Dr Enrico Morselli, professor of psychiatry at Genoa University. Subscriptions are received by Dr P. Celesia, 46 Via Assarotti, Genoa.

The Rev. H. N. Hutchinson (Author of "Extinct Monsters") is at present making a large collection of photographs from life of native races of all countries, including Pacific Islands, to illustrate a popular work which he will publish next year in twenty-four sixpenny parts, with Messrs Hutchinson & Co., of 34 Paternoster Row, E.C. For this purpose he requires good, strong, clear photos of men, women and children—some in groups, others as single figures. Silver prints will be preferred. Readers of *Natural Science* who have such photographs are requested to communicate with Mr Hutchinson, through the publishers, with a view to purchase.

Further Literature Received

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OBITUARIES

GEORG BAUR

BORN, JANUARY 4, 1859. DIED, JUNE 25, 1898

THE premature death of Dr Georg Baur, of the University of Chicago, removes one of the most brilliant investigators of vertebrate morphology from the ranks of the present generation of naturalists. He was born nearly forty years ago at Weisswasser, in Bohemia, where his father was at the time Professor of Mathematics. Most of his youth was passed in Hesse and Würtemberg, and his final school was the Gymnasium at Stuttgart. In 1878 he entered the University of Munich, devoting his attention chiefly to zoology, palaeontology, geology, and mineralogy; and in 1880 he removed to Leipzig, where he studied under Credner and Leuckart. Two years later he returned to Munich to take his degree of Ph.D., and from 1882 to 1884 he acted as assistant to Prof. von Kupffer. Dr Baur was then invited by Prof. Marsh to assist him in his work on extinct vertebrata in the Peabody Museum of Yale University at New Haven; and from 1884 onwards he made his home in the United States of America. He visited England in 1886, and again in 1888, and returned to Germany on several short holidays. In 1890 he resigned his assistantship at Yale, and became lecturer in the Clark University at Worcester, Mass. In 1892 he was appointed Assistant Professor of Comparative Osteology and Palaeontology in the newly-founded University of Chicago; and in 1895 he was finally promoted to the rank of Associate Professor, the position he held at the time of his death. He was always intensely active and absorbed in his favourite studies, and the continuous mental strain eventually led to a serious collapse in health last autumn. A few months' rest seemed to revive him, and he returned to Chicago in December; but he was soon compelled to relinquish work again, and the University granted him an extension of leave to visit his relatives in Germany. Paralysis unfortunately supervened, until his mind became completely deranged, and a wide circle of friends has now to mourn his sad and untimely loss.

Dr Baur's researches related chiefly to two subjects—the skeleton of the vertebrate animals, and the natural history of the Galapagos Islands. He was the author of about 150 notes and papers detailing the results of these researches. He also published the first part of a special work entitled *Beiträge zur Morphogenie des Carpus und Tarsus der Vertebraten* (*Jena*, 1888). He was especially interested in the skeleton of reptiles; and although he often arrived at conclusions too hastily, and frequently changed his views, his papers were always filled with suggestiveness and brilliant generalisation, which none interested in the progress of knowledge could afford to overlook. His writings on osteology, indeed, though brief, are among the classics of the subject. Dr Baur's interest in the Galapagos Islands was first aroused by the extinct giant tortoises of North America and the possibility of their close kinship with those of the distant archipelago off the South American coast. It was not until 1891, however, that he

was able to gratify his desire to examine the facts. In this year he visited the islands himself, leaving in May and returning in October, not only making observations but also amassing a most valuable collection of specimens illustrating the fauna and flora of the group. He published his results in a series of important papers, still incomplete at the time of his death. He specially emphasised his discovery that each island had peculiar species of animals and plants, those of neighbouring islands never quite identical though closely related; and he maintained that this harmonic distribution, as he termed it, could only be explained on the hypothesis, that the Galapagos were the summits of mountains of a submerged continental area, which had once been connected with the mainland of America. This heterodox view was at first much opposed, but it has now met with nearly universal acceptance among naturalists. It was approvingly discussed recently by Dr Günther in his presidential address to the Linnean Society; it has also been admitted as plausible by Mr W. Botting Hemsley, the specialist on insular floras. Dr Baur's work was thus far-reaching in the realm of biological and geological philosophy, and an investigator of his originality and broad ideas can ill be spared.

ANTON KERNER RITTER VON MARILAUN

BORN JUNE 13, 1831. DIED JUNE 21, 1898

DR KERNER VON MARILAUN, whose death at the age of 67 is announced, was Professor of Systematic Botany, and Director of the Botanic Gardens and Museum of Vienna University. He is best known to English students from his "Pflanzenleben," perhaps the most charming work on plant-life ever written; it was recently translated into English under the direction of Prof. F. W. Oliver. Another delightful but smaller work is his "Plants and their Unbidden Guests," translated by Dr Ogle, with a preface by Charles Darwin. These are the work of a keen observer and lover of nature, who was gifted with a vivid imagination, which occasionally led him beyond the limit of strict scientific accuracy. Kerner also wrote several books and papers dealing systematically with the flora of Southern Austria, the Tyrol, and neighbouring countries.

FERDINAND COHN, who, on June 25th, died suddenly at Breslau, was born in that town in 1828, educated there, and had held the chair of botany at its University since 1859. Author of several books and papers dealing with plant-life in general, he was best known to the public by his semi-popular work "Die Pflanze," of which the first edition appeared in 1870, the second in 1896. His more strictly scientific work was on the lower groups of plants, such as Algae, Fungi, and Bacteria, and most of it was published in "Beiträge zur Biologie der Pflanzen," an irregular periodical founded by Cohn in 1870 and edited by him till its cessation in 1896. He also edited the "Kryptogamen-flora von Schlesien," which began in 1876 and is still incomplete.

The deaths are also announced of:—Dr PAUL BROCCHI, professor of zoology and director of the National Agronomic Institute at Paris, and president of the Society of Agriculture; and on March 7th, at Columbia, Mo., of E. H. LONSDALE, of the U.S. Geological Survey, and formerly connected with the Geological Surveys of Missouri and Iowa.

NEWS

Among recent appointments, we notice those of: Mr Herbert Bolton, F.R.S.E., who for the last eight years has held the post of assistant keeper in the Manchester Museum, to the curatorship of the Bristol Museum; Miss Agnes Mary Claypole of Wellesley College, to be assistant in the department of histology and comparative physiology in Cornell University; Dr Chas. H. Judd, of Wesleyan University, to be professor of physiological and experimental psychology in the School of Pedagogy, New York University, being succeeded as instructor of philosophy at Wesleyan University by Dr Raymond Dodge; Dr D. S. Miller to be lecturer in psychology at Columbia University, and Dr E. Thorndyke to a similar post at Western Reserve University.

The new buildings of Reading College were opened by the Prince of Wales on June 11th. He explained that the Institution had for its object the advancement of higher education, especially in those branches more particularly connected with the science and art of agriculture. Reading College is an outcome of Oxford University Extension work, and it is hoped that it may be affiliated to the parent University for the purpose of training students in the science and practice of agriculture as part of their University career. For the present, however, Oxford University has rejected this proposal by a very narrow majority.

Lady Warwick has established a small school of science on her estate at Bigods near Dunmow in Essex. Mr E. E. Hennesey of the Royal College of Science has been appointed principal of the school, which is attended by about sixty pupils of both sexes. The Essex County Council is interesting itself in the movement, and we agree with *Nature* that the experiment in every way deserves success.

The people of Birmingham are no longer satisfied with Mason University College, but wish to have a University of their own in which Technical Science should be one of the faculties. A meeting was held in the city on July 4, and speeches were delivered by Mr G. H. Kenrick, who has promised £10,000, Prof. W. A. Tilden, the Bishop of Hereford, and Mr Joseph Chamberlain.

Dr E. D. Pearsons, of Chicago, has presented $25,000 to Pomona College, California, for the erection of a science building.

The Loubat prizes of Columbia University, awarded every fifth year, have this year been given to Mr W. H. Holmes, of the U.S. National Museum (who receives $1,000), and to Dr Franz Boas, of the American Museum of Natural History (who receives $400), for work on archaeology and ethnology of North America. We note that Mr Joseph F. Loubat has recently presented $1,100,000 to the Library of Columbia University.

Prof. J. Reighard, of Michigan University, accompanied by Prof. H. B. Ward, of Nebraska University, Mr A. J. Pieters, and others, is making a biological examination of Lake Erie.

From the report of the Linacre Professor of Comparative Anatomy we learn that the most important recent additions to the objects exhibited in the court of the Oxford University Museum are a cast of the skeleton of the gigantic extinct reptile, *Iguanodon bernissartensis*, from Brussels; a cast of the skeleton of the extinct theromorphous reptile, *Pareiasaurus baini*, from the Cape Colony; a cast of the five-toed ancestral ungulate, *Phenacodus primaevus*, from the U. S. America; a cast of the skull of the gigantic extinct bird *Phororhacos longissimus*, from Patagonia; and an actual specimen of the extinct shark, *Cladoselache fyleri*, from

near Cleveland, Ohio, which last is intrinsically the most valuable specimen recently added to the collection.

THE Museums Association held its annual conference in Sheffield on July 4-8, under the presidency of Alderman Brittain, who, in his address, gave a history of the growth of Museums in Sheffield. The papers read were :—"Museums in Relation to Art Teaching," by Prof. W. C. F. Anderson, of Sheffield, who might have entitled it "Unfortunate experiences of an Art-Student in search of information"; "The Relation of Museums to Elementary Education," by Prof. A. Denny, who dealt chiefly with object-lessons for teachers drawn from zoological subjects; "A History of the Glasgow People's Palace," by James Paton; "Provincial Museums and the Museums Association," by H. Bolton, a suggestion subsequently endorsed by the Council, that a return of all provincial museums in the United Kingdom, their staff, expenditure, and character should be prepared by a committee; "Museums from a Philistine's point of view," by R. E. Ariel Wright; "Note on some Arrangements and Fittings in the Sheffield Museum," by E. Howarth; "Methods of Preservation and Arrangement of Seaweeds for Exhibition," by Prof. F. E. Weiss; "The Arrangement of Museum Herbaria," by E. M. Holmes, who described various methods adopted in various large establishments; "The Advantages of a Gallery of Sculpture (Casts) to an Art Museum," by J. MacLauchlan; "Notes on some Russian Museums," by F. A. Bather, who used certain museums of St Petersburg, Reval, Jurjew (Dorpat), Moscow, Saratow, Astrakhan, Tiflis, Theodosia, as pegs for various controversial subjects; "The Electric Light Installation at the Manchester Museum," by W. E. Hoyle; "The Cleaning of Museums," by Miss Nordlinger, who described the process as it is and as it should be carried out at the Manchester Museum; "The Individuality of Museums," by William White of the Ruskin Museum; "Marine Animals Mounted as Transparencies for Museums," by Dr H. C. Sorby; "The Ethnological Arrangement of Archæological Material," by Harlan I. Smith; and "The Australian Museum," by its secretary, S. Sinclair. During the meeting visits were paid to the Ruskin Museum, the Sheffield Public Museum, the Mappin Art Gallery, University College, the electroplating works of Messrs John Round & Son and Messrs Walker & Hall; and to the works of Messrs Cammell to see the rolling of an armour-plate for H.M.S. 'Ocean.' The meetings of the Association were held in the Council Chamber of the Corporation, and lunch was kindly provided each day by the Lord Mayor. Hospitality was also offered by the President, by Alderman Gamble, and other residents of Sheffield. The meeting concluded with excursions to the Langsett Water Works under the guidance of the engineer, Mr W. Watts, and to Castleton under the direction of Mr John Tym.

OUR remarks last month with reference to the salary offered to the curator of the Bristol Museum have led a correspondent to send us the *Western Daily Press* for July 13, from which we are glad to see that there are enlightened councillors in Bristol after all. On the recommendation that the salary of the curator should be £200 *per annum*, Mr Saise asked who assessed the value of the curator's services. He thought that the salary was ridiculously inadequate, and that they would shortly be asked to increase it. Mr Stephens suggested that Mr Saise should have the courage of his convictions and move a larger salary. Personally, he thought £200 sufficient to start with. Mr Lloyd said that the Museum only served a small portion of the city, and, considering that fact, he thought the £2000 per annum which was required for its maintenance was too much. Mr Barker was not surprised at Mr Saise having raised the question as to the salary, because to many people the amount would seem inadequate. But the committee were placed in a difficulty, inasmuch as the salary which Mr Wilson received was £200, and a proposal to increase it would have necessitated the committee coming before the Council, and involved delay in the appointment. The idea

that the Museum was too costly would not be borne out by the facts. It was an exceedingly popular institution, and was undergoing changes which would, he trusted, make it even more popular. They were building up a magnificent reference library, of which the city would be proud in years to come.

THE Association of Naturalists of Levallois-Perret has, during the past fourteen years, gathered together the material for a museum, which is open free every Sunday except when the Association is out on an excursion. The collections, says *La Feuille des jeunes Naturalistes*, comprise 5264 species of animals, 4626 of plants, and 4929 geological specimens.

PROF. O. C. MARSH has recently transmitted from New Haven to the Director of the U.S. Geological Survey the fourth large instalment of Vertebrate Fossils secured in the West, in 1882-92, under his direction, as Palaeontologist of the U.S. Geological Survey in charge of Vertebrate Palaeontology. The collection is packed in one hundred boxes, and weighs over thirteen tons. It includes twelve skulls and other remains of the gigantic Ceratopsia from the Cretaceous ; various Dinocerata fossils from the Eocene ; a series of rare specimens of *Brontotherium*, *Elotherium*, *Miohippus*, and other genera, from the Miocene ; a very extensive collection of Rhinoceros and other mammals from the Pliocene ; as well as various interesting fossils from more recent deposits. These will all be deposited in the National Museum of Washington.

THE Government has finally stated that it is unable under existing circumstances to embark upon an undertaking of such magnitude as an Antarctic Expedition. What the Government is unable to do the Royal Geographical Society will attempt. Its Council has authorised the president to take steps to obtain subscriptions to the amount of not less than £50,000, while the Society itself will contribute £5000.

LADIES attending the International Congress of Zoologists at Cambridge in the company of a member may become Associates on payment of 10s.

THE Zoological Society of London has obtained, through Hagenbeck of Hamburg, a male giraffe, aged one year, from French Senegambia. Though at present only eight feet high he is in good condition, and will, it is hoped, ultimately prove a fitting mate for the female giraffe that has been in the Gardens for some years.

THE metric system of weights and measures is to be introduced officially into Russia. The Russians are also considering how they may best abandon their Old Style calendar for that now prevailing in the rest of the civilised world.

AT Rueschlikon, on the Lake of Zurich, the shore for a distance of two hundred metres has sunk into the lake, together with some uninhabited buildings standing upon it. The damage done is considerable.

THE company established at Lubec, Maine, to extract gold from sea-water, and alluded to in our May number, is said by the *Engineering and Mining Journal* of New York to be "simply another attempt to impose on the credulous."

WE notice that a paragraph is going the round of the scientific press, quoting an explanation given in the *Zoologist* by Mr F. R. Godfrey of Melbourne, concerning the sheep-attacking habits of the Kea, *Nestor notabilis* of New Zealand. We do not intend to copy this paragraph into our own pages, because, precisely the same explanation will be found in *Natural Science*, vol. viii., p. 157, March 1896.

THE Government of New South Wales has fitted out a deep-sea trawling expedition for experimental fishing off the coasts of the colony. Mr E. R. Waite of the Australian Museum is attached as naturalist, and much valuable material, including many new species, is finding its way to the museum.

SIR E. BRADDON, Premier of Tasmania, has issued a proclamation protecting the White-capped Albatross for five years from April last.